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CHAMBERLIN'S WORK IN WISCONSIN

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THE peak of accomplishment which marked the closing years of Chamberlin's long life of geologic research had a broad and carefully laid base in his work in Wisconsin, from the time of his graduation from Beloit College in 1866 until 1892, when he went to the University of Chicago. Describing the pioneer Wisconsin conditions under which he began his geological work, in a memorable address before the Wisconsin Academy of Sciences, Arts, and Letters on the occasion of its fiftieth anniversary, he said:

> The larger vision that came with the wider interests and experiences of the war, visions of that which was national rather than personal, entered into the new mental attitude. The man whose pre-war thoughts had centered on his farm, his town, or his county, had been forced to dwell on his state and his country at large and he could not permanently shrink back to his former limitations of interest. The man who had marched shoulder to shoulder could not well relapse into personal isolation. And so the half decade following the war became the generative period of these broader views and those generous instincts of coordination that led to the organization of a common effort for the intellectual development of the state.

In 1869 Chamberlin became professor of natural sciences at Whitewater Normal School. Having no laboratories and collections to work with, he took his students to the surrounding country, and there began the long series of observations and interpretations of nature which continued throughout his life. Even at this early stage he was not content with a mere listing and description of isolated phenomena but strove to cover the ground systematically,

with constant effort toward interpretation and broad correlation of data. He was a natural scientist and remained one throughout his life.

Moving to Beloit as professor of geology, his geologic observations soon extended beyond the local field, and he became an important influence in the organization and conduct of the first Wisconsin Geological Survey, first as assistant state geologist and then as chief geologist.

In the incredibly short period between 1873 and 1882, Chamberlin produced four large volumes on the geology of the state, with the aid of a remarkable group of geologists which he had assembled, including Lapham, Irving, Wright, Strong, Pumpelly, Sweet, Brooks, Wight, Wooster, Van Hise, and Salisbury. In view of the pioneer stage of development of the state, the physical task alone was enormous. Every phase of geology was covered. Since then there has been much additional study of the geology of the state, but the main outlines have stood with little modification. Chamberlin's own investigations were highly varied. Glacial, Paleozoic, and pre-Cambrian geology, artesian wells, the lead and zinc deposits, soils, all came within his sphere of observation. About twenty articles came from his pen during this period, many of which related to glacial problems. Among the others his report and atlas on the lead and zinc deposits of southwestern Wisconsin was an outstanding contribution to economic geology. Not only were the facts presented in his usual masterly style, but his theory of origin of the ores showed daring imagination. The problem is not solved even yet, but it is safe to say that the ultimate solution will contain elements of his theory.

Volume I of the *Wisconsin Geological Survey Reports* was last of the four to be published. This volume is perhaps unique among Geological Survey publications because of the requirements of the enactment that the Survey

> collate the general geology and the leading facts and principles relating to the material resources of the State, together with practical suggestions as to the methods of detecting and utilizing the same, so as to constitute the material for a volume suited to the wants of explorers, miners, land owners, and manufacturers who use crude native products, and to the needs of the schools of the State, and the masses of intelligent people who are not familiar with the principles of

geology; said volume to be written in clear, plain language, with explanations of technical terms, and to be properly illustrated with maps and diagrams, and to be so arranged as to constitute a key to the more perfect understanding of the whole report.

In this volume Chamberlin attempts to meet these difficult requirements. He says in the Preface that "in the endeavor to make the current of thought clear, the attempt has been, not so much to shallow the stream, as to clarify the waters; with what success, the reader must judge." This volume is not only still useful to the people of the state, but as a rounded scientific and literary effort to tell a complete story of the geological development of a state, is yet without a peer.

Chamberlin's absorption in abstruse scientific thought in his later years gives little indication of the shrewdness and organizing ability that marked his great success with the Wisconsin Geological Survey. There were many difficulties to be overcome, funds to be coaxed from the legislature, state officials to be dealt with, staff to be gathered and directed. Never losing sight of his main objective, his imagination always working beyond the field of immediate effort, he had at the same time the executive qualities necessary to direct and drive the project through to completion. It is seldom that there has been such a happy combination in a scientist of first rank.

Still further, Chamberlin never lost sight of the broader relations of geology to the other sciences and to public needs. He was one of the founders of the Wisconsin Academy of Sciences, Arts, and Letters in 1870, and one of its active supporters. Fifty years later his service to the Academy was commemorated in a memorial medal.

The brilliant completion of the first Wisconsin survey brought Chamberlin into public notice, both as a geologist and an administrator. For the succeeding five years he acted as chief of the glacial division of the United States Geological Survey and also for two of these years as professor of geology at Columbian University. During that period his activities extended far beyond the boundaries of Wisconsin.

In 1887 he was called to the presidency of the University of Wisconsin. The five years of his presidency were marked by the

transition of the university from the old classical basis to its modern form. He was responsible for establishing science on a parity with other studies of the university curriculum and for building up the science departments. This reorganization was of far-reaching scope, much greater than any made since, and again he demonstrated his ability to deal with legislators and state officials. Notwithstanding his ability in this field, his heart was in science, and when he had accomplished what he regarded as his principal work of reorganization at the university he resigned to become head of the department of geology in the then newly organized University of Chicago. This was done solely with a view of putting administrative work behind him and being free for scientific investigation. How consistently he lived up to his purpose will be told by others.

Chamberlin's classical education at Beloit College was reflected throughout his life in the dignity and distinction of his speech and writing, but it is an interesting fact that in later years he became impatient of the style imposed by the classical tradition and made a conscious effort toward the use of Anglo-Saxon forms and terms on the ground that Greek and Latin had retarded evolution of language and that the advance of knowledge required new forms of expression.

THOMAS CHROWDER CHAMBERLIN'S CONTRIBUTIONS TO GLACIAL GEOLOGY[1]

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PREDECESSORS AND EARLY DAYS

SO PROMINENT did Thomas Chrowder Chamberlin become in the field of glacial geology that it is a matter of some interest and importance to consider what was the status of the science at the time he turned his attention, as a student and a teacher, to it. So voluminous is the literature of this subject that it is obvious that one cannot in a sketch of this kind give even a general summary of the many books and papers on the subject published in this country, even were it desirable to do so. The intention is to indicate only what were some of the most notable investigations and publications. Some of Professor Chamberlin's most important contributions are undoubtedly woven into the warp and woof of the productions of the numerous men who were closely associated with him and carrying on field observations under his direction. So intimate were these relations that, as the writer himself found, it was sometimes difficult in the end to discriminate between the ideas which the student might feel were his own and those which he had absorbed from contact with the professor.

About fifty years before the birth of Thomas Chamberlin, which occurred in Mattoon, Illinois, in 1843, references to scattered erratics, and what was later recognized to be glacial drift, began to appear in the scientific literature of North America. For a long time this material was regarded as diluvium, or the product of transportation by Noah's flood of biblical note, and many curious explanations were offered as to how this débris was transported and left scattered over hill and dale. The theory of transportation and deposition of

[1] Published by permission of the director of the United States Geological Survey.

The material for this sketch has been taken, very largely, from numerous publications supplemented by the writer's personal acquaintance. Dr. Rollin T. Chamberlin and several other geologists have aided by examining the first draft of the manuscript and giving suggestions.

the drift of Northern Europe by a continental ice sheet, as postulated by Louis Agassiz in 1837, was put on a firm foundation by the publication, in 1840, of Agassiz' *Étude sur les Glaciers*, which set forth the results of his own studies, together with those of Charpentier, Venetz, and Hugi. In 1846 Agassiz came to America and he very soon began the application of the theory of field studies of the drift in the United States. By the time Mr. Chamberlin had completed his undergraduate studies at Beloit College, in Beloit, Wisconsin, in 1866, the caution with which the theory of continental glaciation was received, both in Europe and America, had given place to rather general, though not unanimous, acceptance among geologists. By this time also a great deal of material descriptive of the glacial formations in North America had been published by Edward and C. H. Hitchcock in New England; by Logan and J. W. Dawson in Canada; by the several geologists of the New York Geological Survey; by J. S. Newberry in the region of the Great Lakes; by Orton in Ohio; Alexander Winchell in Michigan; Worthen in Illinois; Owen and White in Iowa; N. H. Winchell in Minnesota, and by Owen, Lapham, Desor, Perceival, and Whittlesey in Wisconsin and the region of the Great Lakes.

TEACHING AND THE WISCONSIN GEOLOGICAL SURVEY

The school year 1868–69 was spent by Mr. Chamberlin in graduate study in several sciences at the University of Michigan, including work in geology with Alexander Winchell, who was then giving his best efforts to promote a higher grade of scientific instruction in the educational institutions of that state. The State Normal School at Whitewater, Wisconsin, where Chamberlin was professor of natural science in the years 1869–73, is at the southern edge of the great Drumlin field of southeastern Wisconsin and in view of the abrupt inner, or ice-contact, face of the range of hills comprising the great Kettle moraine. This range of hills was one of his first and best-loved objects of field study. As the psalmist drew his strength from the hills, so Professor Chamberlin drew inspiration from the Kettle moraine and all that its full explanation implied. While the environment of Whitewater, Wisconsin, stimulated interest in glacial geology, the fact that the young professor occupied a

whole settee rather than a chair in natural science at the State Normal School tended to broaden his foundation for later more specialized scientific studies.

From Whitewater, Chamberlin went to Beloit College, and there he spent the years 1873–82 as professor of geology. The college at Beloit, Wisconsin, stands on the broad terrace top of the great deposit of outwash sand and gravel which fills the ancient valley of Rock River south of the terminal moraine of the Green Bay glacial lobe, and from the campus one looks across the postglacial channel of Rock River to the rolling drift-covered upland to the west, on which was the Chamberlin home farm. The professor was now in the thirties and his active participation in geologic investigations going on in Wisconsin is indicated by his being appointed assistant state geologist in 1873. This position he held until 1876, after which he served as state geologist of Wisconsin until 1882. During this time he continued teaching geology at Beloit.

In 1854 Sir A. C. Ramsay, in England, and A. Marlot, in the Alps, had recognized that the Pleistocene embraced more than one epoch of glaciation, and Oswald Heer, in 1858, had brought to notice evidence of mild interglacial climate found in buried plant remains in the Alps. Such evidence was elaborated later by Professor James Geikie of the University of Edinburgh in the first, second, and third editions of the *Great Ice Age*, and by some other authors in Europe and by Edward Orton, N. H. Winchell, and a few others in the United States.

In the first edition of his *Great Ice Age* (issued in 1874) Geikie presented his ideas that there had been more than one glacial period and one or more interglacial periods, and that a glacial period had intervened since Paleolithic man left his stone implements in the deposits of Southern England. In the second edition he stated his conclusion that the later drift deposits were not the result of submergence and deposition from floating ice, but that there was an actual recurrence of advance of the ice sheets following a milder interglacial period.

The first of Professor Chamberlin's geologic papers was printed in 1875 by the Wisconsin Academy of Sciences, and the first of his publications treating of glacial drift, of which the writer finds men-

tion, was in the *Annual Report of the Wisconsin Geological Survey* for the year 1876 when he was thirty-four years of age. This contribution consisted of three hundred and eight pages, including illustrations, tables, and text on the geology of eastern Wisconsin, and of this forty-eight pages treated of the Quaternary formations, the drift. The report was printed in Volume II, *Geology of Wisconsin*, the first of the four volumes to be published under his incumbency as state geologist. This volume was issued in the same year as the second edition of Geikie's *Great Ice Age* cited in the foregoing. In the descriptions of the glacial phenomena in Volume II, *Geology of Wisconsin*, Dr. Chamberlin did not definitely treat the glacial period as having comprised more than one advance and retreat of the ice. He states in one place[1] that "the survey in the seventies swept over the Pleistocene formations and those that underlie them at the rate of about 4,000 square miles a year and thus perforce had something of the nature of a bird's-eye view." His studies of the Kettle moraine in eastern Wisconsin and its continuation in other states, however, led him to important conclusions and, in 1878, he published in *Transactions of the Wisconsin Academy of Sciences* a paper entitled, "The Extent and Significance of the Wisconsin Kettle Moraine." In this he states:

> The moraine constitutes a definite historical datum line in the midst of the glacial epoch and becomes a basis of reference and correlation for adjacent formations. It is an historical rampart outlining the great dynamic agency of the period at an important stage of its activity and separating the formations on either hand by a chronological barrier. . . . .
>
> If the evidence adduced to show that the Kettle moraine was due to an advance of the glaciers be trustworthy, then to the extent of that advance, whether much or little, the moraine marks a secondary period of glaciation, with an interval of deglaciation between it and the epoch of extreme advance. Its great extent indicates that whatever agency caused the advance was very widespread if not continental in its influence. The moraine, therefore, may be worthy of study in its bearings upon the interesting question of glacial and interglacial periods.

In Volume I of the *Geology of Wisconsin* (the last of the set of four volumes prepared under his direction), which was submitted for publication in June, 1882, and issued in 1883, Chamberlin

[1] Preface to "The Quaternary Geology of Southeastern Wisconsin," by Wm. C. Alden, *United States Geological Survey Professional Paper 106* (1918), p. 13.

inserted a discussion of the "second glacial epoch" and a note explaining why he had, in this volume, divided the glacial period in a way not done in the earlier volumes. He wrote[1]

In the descriptive volumes of this report, two distinct glacial periods are not formally stated, although the fact of a second advance, with an intervening interval, is indicated. This was due partly to the fact that investigations were still in progress, which made it injudicious to prejudice results by broad conclusions in advance of the fullest available data, and partly to the fact that the existence of two such periods had not been generally recognized by American geologists, although the doctrine of separate glacial periods had been entertained by several in this country, following the lead of the Scotch school. The only American evidence then adduced, aside from theoretical presumptions, consisted of supposed superpositions of newer upon older till, separated by supposed interglacial deposits—a class of evidence to be received with great caution, since temporary oscillations, or the shifting of subglacial streams, may produce strikingly analogous phenomena. Where the section exposed to observation chances to be parallel to the glacial margin or the course of a subglacial stream, the phenomena may seem to be much more prevalent than is really the case. A further and more important ground of doubt arises from the fact that certain subaqueous deposits so closely resemble true till that they have been mistaken for it, and there is perhaps no case of superposition of beds supposed to represent two glacial periods that is not still open to these doubts. Our present firmness of conviction arises (1) from the discovery and working out of an extended moraine stretching across the whole of the glaciated area and belonging to a system of glacial movements which differ in many important respects from the earlier ones; and (2) from the differences of surface contour due to the greater erosion of the earlier, as already indicated. We believe that this line of evidence, when developed in its fulness, will prove entirely demonstrative. Only a small part of the results now gathered fall specifically within our present province as chronicler of the geological history of Wisconsin, but the total result is, in some important measure, the outgrowth of investigations begun in this State.

The subdivisions of the Quaternary age he presented as follows:

Quaternary age	Terrace or Fluviatile epoch
	Champlain or Lacustrine epoch
Glacial period	Second glacial epoch
	Interglacial epoch
	First glacial epoch

The method of his treatment was used, as he wrote, "for the sake of ease and simplicity of description, rather than from a disposition

[1] *Geology of Wisconsin*, I (1883), 271–72.

to ignore opposing opinion." His discussion may be summarized as follows:

The ice of the first glacial epoch was portrayed as entirely surrounding but not overrunning the great Driftless Area. The extent of the ice of his second glacial epoch, as shown for the Upper Mississippi Valley, approximates the areas at present known to be covered by drift of middle and late Wisconsin age. The movements of the ice were shown to have taken place "without regard to the local slope of the surface." The final retreat of the ice was attended by oscillations of the glacial front as represented by the formation of later secondary moraines. The drift-buried vegetal deposits, the so-called "forest beds" of Wisconsin, were stated to "belong to at least three epochs, one of which is the interglacial one here noted."

Following Agassiz, Forbes, and Tyndall, the flowage of glacier ice was said to be "essentially similar to that of viscous fluids." He found a striking accordance between the glacial phenomena of Wisconsin and Croll's hypothesis as to the cause of the glacial climate. He expressed grave doubt as to the competency of both preglacial and glacial erosion combined to have produced the basins of the Great Lakes, believing that one element was "a subsidence of their bottoms due to glacial occupancy."

GLACIAL GEOLOGY AND THE FEDERAL SURVEY

In 1879 the United States Geological Survey was established and in 1881–82 Dr. Chamberlin, then about thirty-eight years of age, began under its auspices studies of the drift in the various states. He continued as geologist in charge of the glacial division until 1904, and summaries of the glacial investigations under his direction appear in the successive annual reports of the director of the Geological Survey. During the years 1882–87 he resided in Washington, D.C., except when absent on field studies, and in 1885–87 he served as professor of geology at Columbian (now George Washington) University in the capital city.

Professor Chamberlin now continued his studies of the extent and relations of the Kettle moraine and the several associated moraines, which he had already begun in states other than Wisconsin. In this work he associated others with him: Professor J. E. Todd in the area between the Mississippi and the Missouri rivers, and Professor L. C. Wooster in Michigan; he himself also worked in Ohio and New York in an attempt to make connections with the eastern extension of the moraine which had previously been traced

across New Jersey and along the southern coast of New England by Cook, Smock, Upham, and King and, on Long Island as early as 1842, by Mather. The results of these and previous studies in the several states from Dakota Territory to the Atlantic Coast were brought together in a notable "Preliminary Paper on the Terminal Moraine of the Second Glacial Epoch," published in 1883 in the *Third Annual Report of the United States Geological Survey*. In this article tentative correlation was made eastward through the upper Susquehanna basin and the Finger Lakes region of New York, and thence to the Catskills and south, i.e., far back of the "extreme terminal moraine" which H. C. Lewis and G. F. Wright had mapped along a line trending southeastward across northeastern Pennsylvania from Olean, New York, to the Delaware near Belvidere, New Jersey, and which Chamberlin was then inclined to regard as having been formed at an earlier epoch of glaciation. He, however, definitely indicated that these correlations and interpretations were tentative and might require later revision. The character of the mapping between Montana and the Atlantic Coast, sketched by the author of this paper in the *Third Annual Report*, may readily be seen by comparison of these maps with the later ones. Much more complexity of detail has since been worked out, yet the importance of this paper was very great at the time of its publication. Professor Chamberlin had shown that his "terminal moraine of the second glacial epoch" lay far within the outer limit of the drift throughout much of its course across the Mississippi basin. The idea of duality of the glacial period was not, however, accepted by all geologists.

During the later 1880's Dr. Chamberlin visited many parts of the glaciated field and he also examined the loess and "orange sand" of the Lower Mississippi Valley, finding in the latter no evidence of glaciofluvial origin. He continued his studies in the Dakotas with Todd and King and extended his own reconnaissance as far as the Puget Sound region. He also enlisted many others in glacial studies under the auspices of the federal survey: R. D. Salisbury examined the Driftless Area of the Upper Mississippi Valley and the outer border of the drift in Missouri; G. F. Wright mapped the limit of the drift between the Mississippi River and western Pennsylvania, and studied the terraces of the Allegheny; G. H. Stone studied the

osars of Maine, and William Davis the drumlins of Massachusetts, Connecticut, and New York; I. M. Buell traced the boulder trains of Wisconsin; J. E. Todd mapped the glacial deposits in the Dakotas, Iowa, and Nebraska; Warren Upham carried on a comprehensive study of glacial Lake Agassiz; N. S. Shaler studied Mt. Desert Island, Maine, Nantucket, and Marthas Vineyard; J. C. Branner worked in Ohio and eastern Indiana. In 1886 Mr. Frank Leverett began in northeastern Illinois, under the supervision of Professor Chamberlin, the notable studies of the glacial deposits for the United States Geological Survey, which he has continued almost without break to the present day.

From these various studies came numerous papers and important monographs by these scientists, and also short papers and discussions by the geologist in charge.

In 1888, after Dr. Chamberlin had become president of the University of Wisconsin, there was published in the *Seventh Annual Report of the United States Geological Survey* his paper on "The Rock Scorings of the Great Ice-Invasions" in which he illustrated, and drew inferences concerning, the modes of glacial action from a remarkable variety of such glacial scorings. A map issued with this paper shows the extent of the earlier and later glaciations throughout the United States as then known. In the later 1880's and early 1890's Chamberlin himself did little more in glacial geology on the federal survey than direct the studies being carried on by Upham, Todd, Stone, Buell, Wooster, Salisbury, and Leverett. Most of these men were working on their reports when not engaged in teaching or otherwise, and only Upham and Leverett gave their full time to United States Geological Survey work.

Dr. Chamberlin took an active part in the discussion of all phases of the glacial problems at the meetings of the Geological Society of America for many years after the foundation of the society in 1889. He was particularly vigorous in his opposition to the idea that there was but one period, or epoch, of glaciation and to the interpretation of the attenuated drift outside the moraine in Pennsylvania as a so-called "fringe" or dependency of the moraine. This outer drift he regarded as the product of a glacial epoch distinctly older than that which formed the moraine. As stated in the *Bulletin of the*

Geological Society of America, Volume I, he found evidence concerning the interval between the glacial epochs in the following:

a) Considering the loess of the Mississippi Valley as waterlaid, he concluded it originally extended clear across from side to side and that the inner valley, three hundred feet deep and sixty miles wide, from Cairo, Illinois, to the Gulf, had been eroded in interglacial time, before it received the filling of silts washed out from the ice of the later epoch.

b) The cutting of the inner gorge of the Allegheny River, about two hundred fifty feet deep, below the level of rock terraces which carry outwash gravel he correlated with the older drift.

c) Similar evidence was also found on the Susquehanna and Delaware rivers.

In 1890 Chamberlin's interpretation of the terraces of the Allegheny River was published in an introduction to *United States Geological Survey Bulletin 58*, entitled "The Glacial Boundary in Western Pennsylvania, Ohio, Kentucky, Indiana, and Illinois," by G. F. Wright, in which the latter sets forth quite different ideas. One of the hypotheses set forth in this bulletin which Professor Chamberlin strongly opposed was that the deposits on the upper terraces were due to an ice dam five hundred fifty feet high, formed when the glaciers crossed the Ohio into Kentucky in the vicinity of Cincinnati. Such a dam, Wright concluded, had ponded waters to heights of three hundred feet or more above the river level in the Upper Ohio Valley and caused the tributaries to make the high terrace deposits, some of them on rock-cut terraces, which he considered too old to be of Pleistocene age. Chamberlin thought temporary and partial blocking by such an ice dam "was an extremely probable and frequent occurrence but not a permanent damming."

In 1891 Chamberlin and Salisbury presented their interpretations in an article entitled "On the Relationships of the Pleistocene to the Pre-Pleistocene Formations of the Mississippi Basin South of the Limit of Glaciation."[1] The following is a brief summary of important points set forth in this paper:

No soil or weathered zone was found at the top of the drift and below the loess in southeastern Illinois and southwestern Indiana. The loess is an aqueous

[1] *American Journal of Science*, XLI, series 3 (1891), 359–77.

deposit formed immediately after the till at the closing stage of the first glacial epoch. This epoch had two episodes with "forest bed" formed between them. The loess is unconformable on the "Orange sand." The latter is deeply weathered from long pre-loess exposure. There are no drift pebbles in "Orange sand." The "Orange sand" is pre-Pleistocene. Some indications were seen at a few places of a fluvial "loess" of the first episode of the first glacial epoch. There are two loesses in Illinois, Kentucky, Tennessee, Arkansas, and Missouri. There was depression and slack water to cause the loess deposition, and re-elevation and erosion. Later glacial terraces slope down to and disappear under the filling of the Lower Mississippi.

In a paper on the cause of glaciation,[1] presented before the Geological Society of America, Professor Chamberlin rejected Croll's astronomical hypothesis and also high elevations as causes. He suggested that there might have been such a change in the axis of rotation of the earth as to bring the glaciated areas into high latitudes. In a "proposed genetic classification of Pleistocene glacial formations," presented before the International Geological Congress, he grouped loess with "formations produced by glacial waters after their issuance from Pleistocene glaciers" and eolian loess with "formations produced by winds on Pleistocene glacial and glacio-fluvial deposits under the peculiar conditions of glaciation."

In 1892, at about forty-nine years of age, Dr. Chamberlin became head of the department of geology at the newly organized University of Chicago and, gathering about him there a select faculty and a rapidly growing body of students, he found conditions congenial for the study and teaching of the fundamentals of geology and particularly of glacial geology. Here the *Journal of Geology* was founded in 1893, with himself and his colleagues as editors. This journal formed a convenient medium of publication and among its special articles and editorials are many of his own treating of various problems of glacial geology. A scanning of its pages affords a ready means of access to his most notable studies for the next thirty-five years.

MAN AND THE GLACIAL PERIOD

Professor Chamberlin was much interested in the subject of the time of man's appearance in America, but he did not regard as valid the evidence brought forward by several scientists that primitive

[1] T. C. Chamberlin, "The Present Standing of the Several Hypotheses of the Cause of the Glacial Period," *American Geologist*, VIII (1891), 237 (abstract).

Paleolithic man reached North America prior to the last glaciation. Together with R. D. Salisbury and W. H. Holmes, he regarded the so-called "paleoliths" found in North America as merely the rejects from the manufacture of better stone implements. He published articles by Professor Holmes in the *Journal* and had him instruct students in geology at the university in the critical scrutiny of the conditions under which artifacts became imbedded in glacial and glaciofluvial deposits, and how to interpret them. Such scrutiny led him later to reject the interpretations placed by some on the buried human remains found near Lansing, Kansas (1902), and Florence, Nebraska. In his discussion of the geologic relations of the human relics of Lansing, Kansas,[1] he gave what Dr. Samuel Calvin[2] characterized as a

> full and clear presentation of the behavior of rivers of the Missouri type in connection with migrations of their meanders, of their work in degradation and aggradation, in scour-and-fill, while deepening and widening their valleys, and of the changing conditions which they impose on their tributaries.

By 1893 Professor G. F. Wright, on the basis of his own interpretations, had come to agree with Chamberlin and Salisbury, that the ice had extended far beyond the terminal moraine in New Jersey, but he did not regard the outer drift as of an older glacial epoch. The Chicago men vigorously opposed Wright's interpretation that the material was already weathered when it became incorporated in the drift. Professor Chamberlin (1894), in a review of J. W. Dawson's *Canadian Ice Age*, dissented radically from that author's fundamental conclusions. Dawson's conception of the Pleistocene included:

a) Earlier Pleistocene—Laurentian and Greenland ice sheets and local glaciers.

b) Middle Pleistocene—a mild interglacial epoch.

c) Later Pleistocene—glaciers in mountains, with the interior plains submerged and the later drift being dropped from icebergs.

CLASSIFICATION OF THE QUATERNARY DEPOSITS OF NORTH AMERICA

In 1894 the third edition of James Geikie's *Great Ice Age* was published in Europe with a map and two chapters by T. C. Cham-

[1] *Journal of Geology*, X (1902), 745–77. [2] *Op. cit.*, p. 777.

berlin on the glacial phenomena of North America, and in this very important description names were suggested, for the first time, for the different stages, as the writer there designated them. He gave two different possible groupings on a twofold basis, together with the following grouping on a threefold basis, which he regarded as, in many respects, the most satisfactory interpretation from the glacial deposits themselves:

GLACIAL PERIOD

1. Concealed under series (theoretical)	Unknown.
2. Kansan stage of glaciation	First (represented) glacial epoch.
3. First interval of deglaciation	First interglacial epoch.
4. East-Iowan stage of glaciation	Second glacial epoch.
5. Second interval of deglaciation	Second interglacial epoch.
6. East-Wisconsin stage of glaciation	Third glacial epoch.
7. Later oscillations of undetermined importance	Third glacial epoch embracing possibly a fourth glacial epoch.

Neither Geikie nor Chamberlin supported, in this volume, any one definite theory as to the cause of glaciation. The attitude of both may be indicated by one of Geikie's closing sentences as follows: "The primary cause of those remarkable changes is thus an extremely perplexing question and it must be confessed that a complete solution of the problem has not yet been found."

In an article in the *Journal of Geology*,[1] James Geikie discussed the "classification of the European glacial deposits," and suggested names for six glacial epochs and five interglacial epochs, and Dr. Chamberlin printed an article in the same number of the *Journal* on the "classification of the American glacial deposits," suggesting parallelism, rather than correlation with the European deposits, and giving the names as follows:

1. Kansan formation (=Saxonian?).
2. Aftonian formation (=Helvetian?).
3. Iowan formation (=Polandian?).
4. Toronto formation (=Neudeckian?).
5. Wisconsin formation (=Mecklenburgian?).

Chamberlin did not maintain that the three glaciations were of equal importance or that the two deglaciations were equal. He had,

[1] Vol. III (1895).

in 1894, presented in the *Journal* a "genetic classification of the Pleistocene glacial formations."

In 1892 the present Geological Survey of Iowa was organized, with Dr. Samuel Calvin as state geologist, and began study and mapping of the state by counties. This involved detailed study of the glacial deposits, including those in northeastern Iowa which had previously been studied by W. J. McGee. Dr. Chamberlin was greatly interested in these studies, as also in those of McGee, and he was rather closely associated therewith in consultation and field conferences. By 1895 these studies had resulted in an important change in the classification of the drift deposits of Iowa. Concerning this Chamberlin wrote in an editorial in the *Journal of Geology* in part as follows:

> The studies of the past two years seem to show that within the limits of the series covered by the three names first proposed, there is, probably, need for some extension and revision. This arises chiefly from the progress made by the geologists of the Iowa survey, Messrs. Calvin, Bain, Norton and Beyer, and by my colleague, Mr. Leverett. It will be recalled that in eastern Iowa the elaborate investigations of Mr. McGee, some years ago, demonstrated the existence of two sheets of till, separated by a vegetal horizon. It was known that in southern Iowa there were also two sheets of till separated by a vegetal horizon, but these had not been studied in detail nor their connections traced out. It was natural, as well as prudentially conservative, to suppose that these two series were mutual equivalents, as they stood in much the same geographic relationship to the later (Wisconsin) drift. It was recognized that the amount of erosion upon the south Iowan series was greater than that upon the east Iowan, and also that the loess in eastern Iowa was intimately connected with the upper till sheet, while the upper till sheet in southern Iowa was separated from the loess by a definite interval, but the importance of these differences was not fully appreciated. The investigations of the Iowa geologists have led to the quite firm conviction that the upper till sheet of the series in southern Iowa is the lower member in eastern Iowa. They have also become convinced that the upper sheet in southern Iowa extends continuously across northwestern Missouri into Kansas, and is the equivalent of the drift sheet that covers the northeastern part of Kansas. State Geologist Keyes of Missouri concurs in this view. They do not hold this to the exclusion of a possible lower member in Kansas. In harmony with these views the upper till in the southern part of Iowa has been designated Kansan in the recent Iowa reports.
>
> During the past summer I have had the pleasure of making two excursions with Mr. Bain of the Iowa survey to localities where the above formations are advantageously exhibited, and I have been impressed with the cogency of the

arguments of the Iowa geologists. While, therefore, the case cannot be said to be demonstrative, as yet, it seems best to accept the application of the nomenclature adopted by the Iowa survey. This places the Aftonian beds below the Kansan series instead of above them. It puts the sub-Aftonian sheet of till in an earlier category, and, for the present, it may perhaps be regarded tentatively as Albertan, although, of course, it cannot now be demonstrated to be equivalent to the Albertan beds of Canada. The studies of Mr. Leverett have made it quite sure that the Kansan ice-sheet crossed the Mississippi and invaded Illinois to some moderate distance. He has also shown that the Illinois ice-sheet returned the compliment and invaded Iowa. Between these invasions there was a considerable interval of time, as indicated by the greater erosion of the Kansan deposits and by the prevalence of a soil horizon and of peat beds between the Kansan and Illinois till sheets where they overlap. He has shown also that there was a notable interval between the invasion of Iowa by the Illinois ice-sheet and the spreading of the loess over its deposits, as indicated by erosion and the formation of a soil horizon. This loess mantle seems to be identical with that which is intimately connected with the east Iowan drift sheet. It thus appears that the invasion of the Illinois ice marks a distinguishable stage of glaciation separated by a notable interval from both the earlier Kansan stage and the later Iowan stage. This interval appears to be of such moment as to make it inadvisable to correlate the Illinois drift sheet with the Iowan drift sheet. As a result, the practice of designating the former the Illinois sheet has already sprung up among us. The evidence at present seems sufficient to justify its tentative use in the literature of the subject. It should of course be credited to Mr. Leverett.

The series of the Mississippi basin, as thus modified, would be as follows in stratigraphic order:

9. Wisconsin Till Sheets (earlier and later).
    8. Interglacial deposits (Toronto perhaps).
7. Iowan Till Sheet.
    6. Interglacial deposit.
5. Illinois Till Sheet (Leverett).
    4. Interglacial deposit (Buchanan of Calvin).
3. Kansan Till Sheet.
    2. Aftonian beds, Interglacial.
1. Albertan Drift Sheet (Dawson).[1]

In the same editorial there was printed the following memorandum, which has been used many times since:

. . . . While returning from my last visit to the field in which the Kansan, Illinoian, Iowan, and Wisconsin formations were seen in close succession, I made a memorandum of impressions respecting their relative ages simply as a means of comparison with judgments formed at other times, the impressions

[1] *Journal of Geology*, IV (1896), 872–74.

being derived from the respective degrees of erosion and chemical change which the formations have undergone. Although this was intended to be nothing more than a record of passing impressions, it may be the best means of giving some notion of my rating of the historical importance of the formations. Taking the interval from the late Wisconsin deposits (as found immediately south of the Great Lakes) to the present date as unity, the following is the memorandum:

From the close of the later Wisconsin to the present	1 unit
From the earliest Wisconsin (Shelbyville moraine) to the present	2½ units
From the Iowan to the present	5 units
From the Illinois invasion of Iowa to the present	8 units
From the Kansan to the present	15 units
From the sub-Aftonian (Albertan) to the present	x units

Four of the investigators previously named who have seen this memorandum are disposed to increase the figure for the Kansan, and some of them would alter other figures in the same direction, with perhaps a reduction of the estimate for the Iowan. After the estimate had been made it was observed that the intervals form a symmetrical diminishing series. The temperature variations of the period might therefore be represented by an oscillating curve with declining waves.

Perhaps in no way was Professor Chamberlin's profound interest in (one might almost say affection for) the glacial formations of his homeland, the Upper Mississippi Valley, more manifest than in the matter of the classification and nomenclature of the drift sheets which he had done so much to establish. This was particularly impressed upon the writer on the occasion of a last visit early in June, 1928, when the old doctor sat back in his evening chair and, with considerable feeling, recounted the circumstances attending the development of the Pleistocene classification of Iowa. He never became reconciled to the shifting of the name "Kansan" from the sub-Aftonian till to the super-Aftonian till nor the limiting of the name "Iowan" to the thin uppermost till, in northeastern Iowa, McGee's original area. Even to his dying day, and long after the altered nomenclature had become well established in geologic literature, he cherished the hope that the name "Iowan" would be restored, as he thought it should be, to the great drift sheet which is the upper till throughout the southern half of Iowa, and which extends thence into Missouri, Kansas, and Nebraska. It seemed to him that, when a relatively thin sheet of drift was differentiated in northeastern Iowa as later than the great statewide sheet of super-Aftonian till,

it should have been given another name. It was to him as though the state had in some very real sense been bereft of an important feature of its birthright. In the *Journal of Geology*,[1] in a review of an article by Mr. Leverett, Dr. Chamberlin gave an expression of his opinion of the classification, in part, as follows:

> The name Kansan has been shifted from the sub-Aftonian till to the super-Aftonian till originally called Iowan. The term Iowan thus displaced has been transferred from the middle drift to the uppermost and least member of the grouped beds originally covered by them. The reason for these shifts seemed cogent to the workers in the Iowan field at the time they were made and perhaps seem so still. They were accepted with slight reluctance by the glacialist who had given the terms their original applications. The cogency of the reasons for the changes has, however, from his point of view, largely disappeared with the progress of study, and if it were practicable to return essentially to the original usage, making the sub-Aftonian till Kansan and the super-Aftonian till Iowan, and to take the exceptional exposures of both formations near Afton Junction, Iowa, as the types, as was originally done, it would seem to him to accord best with the inherent fitness of the case. Particularly does this seem so in the application of the term Iowan, for the super-Aftonian till not only has a broader and more distinctive expression in Iowa than anywhere else, but it is the greatest of Iowa's drifts; it is inherently the Iowan drift. . . . .
>
> This sketch, even in its incompleteness, may serve to give a measure of historic insight into the embarrassments that attend the correlation and nomenclature of the American middle drift.
>
> Respecting the newer divergencies of opinion implied in Mr. Leverett's paper, which involve the suggested dismissal of the Iowan altogether, as a distinct formation, or else its grouping under the Illinoian, it is appropriate here to urge restraint, patience, and equipoise, for the distinguishing phenomena, while pronounced and peculiar, are subtle in their gradations and singularly puzzling.

In 1894 Mr. Leverett, working under Chamberlin's supervision, had differentiated a drift sheet in southeastern Iowa, which appeared to be younger than the drift exposed farther west. From its lithologic composition he concluded that this drift was deposited by ice which came from the center of glaciation on Labrador peninsula and which crossed the channel of the Mississippi River and invaded Iowa from the east. This was found to be the uppermost till sheet throughout most of that part of Illinois outside the limits of the Wisconsin drift, so the name "Illinoian drift" was chosen as an appropriate designation.

[1] XVIII (1910), 473.

In 1898 Mr. Leverett[1] had proposed the names Yarmouth, Sangamon, and Peorian for three of the interglacial stages, so that (omitting the later substages) the Pleistocene classification as presented by Mr. Leverett in Monograph 38, *The Illinois Glacial Lobe* (1899), was as follows:

OUTLINE OF THE DRIFT SHEETS AND INTERVALS

Stage 1.—Oldest recognized drift sheet—the Albertan of Dawson, including, also, the sub-Aftonian of Chamberlin.

Stage 2.—First interval of deglaciation—Aftonian of Chamberlin.

Stage 3.—Kansan drift sheet of the Iowa geologists.

Stage 4.—Second interval of recession or deglaciation—Yarmouth of Leverett.

Stage 5.—Illinoian drift sheet.

Stage 6.—Third interval of recession or deglaciation—Sangamon of Leverett.

Stage 7.—Iowan drift sheet and main loess deposit.

Stage 8.—Fourth interval of recession or deglaciation—Peorian of Leverett, possibly equivalent to the Toronto Formation of Chamberlin.

Stage 9.—Early Wisconsin drift sheets.

Monograph 38 was the third of several monographs on Pleistocene phenomena to be completed under Dr. Chamberlin's supervision. The first of these works was Monograph 25, *The Glacial Lake Agassiz*, by Warren Upham, issued in 1896, and the second was Monograph 34, *The Glacial Gravels of Maine and Their Associated Deposits*, by G. H. Stone, issued in 1899. All of these monographs of the United States Geological Survey were notable publications presenting, as they did, the results of years of field mapping and study. In a review of Monograph 38, Dr. Chamberlin wrote in part:

> In the matter of classification, the monograph presents the latest and fullest expression of the conclusions toward which investigations in the interior have been steadily tending for the past decade. The classification offered is not regarded as final, either in the sense of including all the possible great divisions, or in the complete characterization of those recognized, but it clearly lies in the line of a true and ultimate classification. Fifteen stages are recognized, six of which are based upon notable glacial advances, five represent notable intervals of deglaciation, and four are based upon lacustrine stages after the beginning of the abandonment of the region by the last ice-sheet. The age of the oldest glacial formation is regarded as many times that of the latest; and the oldest

[1] *Journal of Geology*, Vol. VI.

interglacial intervals are also believed to be many times longer than the later ones. In a word, the oscillations appear to have been large in the earlier stages and to have grown less and less during the progress of the period. This newer view of the relative ages of the successive epochs, sustained as it appears to be by the progress of research in Europe, must be looked upon as one of the most important advances of recent years, for it affects profoundly nearly all of the larger questions of glacial history.[1]

ORIGIN OF THE LOESS

For a long time there had been dissatisfaction among geologists with the explanation of the great body of loess in the Mississippi Valley as of aqueous origin. Professor B. Shimek, of the University of Iowa, in particular, as a result of his extended studies opposed that idea on the grounds of fossil content, texture, and topographic distribution. He interpreted the loess as an eolian deposit formed, not in close connection with glaciation, but under interglacial conditions similar to those of the present day.

In the *Journal of Geology*,[2] Professor Chamberlin presented a "Supplementary Hypothesis Respecting the Origin of the Loess of the Mississippi Valley." In this he cited some of the grave difficulties encountered by a theory of purely aqueous deposition of the main loess deposit. The observed relations, however, seemed to him "to force the conviction that the loess had its origin in some relationship to the Iowan stage and to the rivers that led away from the ice edge at that time." He adopted the glaciofluvial hypothesis "as the fundamental explanation of the origin of the Mississipian loess," and postulated low slope, extensive silt flats, and periodic floods for the "accumulation of fluvial loess, alternating with dry periods, when dust was being swept up by the winds from these flats and lodged in the vegetation on the adjoining lands where land mollusks were living." He held that "there must be an accommodation between the breadth and fluctuation of the fluvial deposits and the extent and massiveness of the eolian deposits."

CHANGES IN PLEISTOCENE CLASSIFICATION

After some question had been raised as to the applicability of Dawson's name "Albertan" to drift underlying the Aftonian inter-

[1] *Journal of Geology*, VIII (1900), 367–68. [2] Vol. V (1897).

glacial beds, the more or less indefinite names "pre-Kansan" and "sub-Aftonian" continued to be used and in their *Geology*[1] and their *College Geology*;[2] Chamberlin and Salisbury used also the name "Jerseyan" taken from the oldest drift in New Jersey. It was stated in the 1906 volume[3] that "this older drift may not improbably be the equivalent of the sub-Aftonian of Iowa, but as direct connection cannot be traced the correlation is uncertain." In 1909, while studying the glacial and interglacial deposits of western Iowa and eastern Nebraska, Professor Shimek[4] proposed the name "Nebraskan" for the pre-Kansan drift, and this convenient name has come into use by many geologists. Its selection, however, seemed to Dr. Chamberlin, for reasons stated in the *Journal of Geology*,[5] to be "of doubtful wisdom" and he did not favor its use.

Some question having arisen as to the distinctiveness of the drift now called "Iowan" and as to its relative age, a co-operative arrangement was made under which the present writer, William C. Alden, for the United States Geological Survey, and M. M. Leighton, for the Iowa Geological Survey, spent most of the summer seasons of 1914 and 1915 in a field study of the Iowan drift of northeastern Iowa and its relations, and their report was published in 1917 in the *Iowa Geological Survey*.[6] On page 56 of that volume the authors stated that "the conclusion has been reached that there is what seems to the writers to be good evidence of the presence of a post-Kansan drift sheet in northeastern Iowa and that this drift appears to be older than the Wisconsin and younger than the Illinoian drift."

Many, if not most, of the geologists who have given any serious thought to the matter appear to be satisfied that there really is such a distinct drift sheet in northeastern Iowa, but there is still some difference of opinion as to its appropriate classification and nomenclature. It may not be out of place to give here the general classi-

[1] III (1906), 383 ff.

[2] Part II (1909), p. 840. [3] P. 384.

[4] B. Shimek, "Aftonian Sands and Gravels in Western Iowa," *Bulletin Geological Society of America*, XX (1909), 408.

[5] XVIII (1910), 294–95.

[6] XXVI, 49–212.

fication in use at present by the United States Geological Survey,[1] and in the building up of which T. C. Chamberlin had a much larger part than is indicated by the citations attached. This classification seems to the writer to serve the purposes of the present state of our knowledge fairly well. Some geologists, as did Dr. Chamberlin, prefer a somewhat different classification and undoubtedly with the increase of our knowledge modifications will be made. Even now local names are given various glacial and associated deposits when there is uncertainty as to their correlation with the deposits of the Mississippi Valley.

STUDIES ON GLACIER MOTION IN GREENLAND

In the summer of 1894 Dr. Chamberlin went to Greenland as geologist to the Peary Auxiliary Expedition and in the following December he took "Glacial Studies in Greenland" as the topic of his address as president of the Geological Society of America. Later a series of notable articles on the same subject were published in the *Journal of Geology*.[2] Among the important points of his observations on the glaciers, discussed briefly in the presidential address, were the following:

Vertical ice fronts due to the sun's low angle of incidence. Stratification due to snowfalls. Dirt layers confined to the lower part of the ice. Introduction of débris by shearing radically different from the results of viscous flow and more like rigid thrust. The progressive growth of the ice granules a most important factor in glacier motion. As ice melts under pressure and refreezes on release of pressure, a granule may lose in one part and gain in another and thus move on its neighbors. The observations on crystallization of the ice oppose viscosity as an essential agency in the motion. A glacier does not push a moraine but

[1] With the omission of some of the later substages, the following is the classification in use at present by the United States Geological Survey:

PLEISTOCENE EPOCH

9. Wisconsin stage of glaciation (of Chamberlin).
   8. Peorian stage of deglaciation (of Leverett).
7. Iowan stage of glaciation (of Iowa geologists).
   6. Sangamon stage of deglaciation (of Leverett).
5. Illinoian stage of glaciation (of Leverett).
   4. Yarmouth stage of deglaciation (of Leverett).
3. Kansan stage of glaciation (of Iowa geologists).
   2. Aftonian stage of deglaciation (of Chamberlin).
1. Nebraskan stage of glaciation (of Iowa geologists)
   (pre-Kansan of Chamberlin) (Jerseyan of Eastern United States).

[2] Vols. II–V, inclusive.

rides over it. Observed daily rates of ice advance 0.4 to 2.78 feet. Average very slow. Not much recent advance or retreat. Greenland ice never advanced much if any beyond the present coast. Former elevation of Greenland was not coincident with conditions favoring glaciation.

Some of the results of his observations in Greenland were later incorporated in "A Contribution to the Theory of Glacial Motion."[1] In a review of another paper,[2] he stated the real proposition involved in the "granular theory," as follows:

I. Glaciers are formed of individual crystalline granules. II. These are controlled by a strong crystalline force. III. They are, however, subject to growth and decadence, resulting in the extinction of some crystals and the enlargement of others, and in changes of relations to one another. IV. The crystals of any part seem to be the enlarged or reduced descendants of those in the part above. V. Collectively they persist throughout the whole glacial movement, and individually they seem to persist through some notable part of it, at least. VI. They participate in the changes of form and the changes of attitude involved in the glacial movement.

The general nature of glacier motion has also been described as follows:[3]

The growth of ice granules, together with slight movement and adjustment between the granules under changing pressure conditions, is considered to be the underlying basic phenomenon. Compression produces heat and at the same time lowers the melting-point of ice. Compression becomes greatest at certain points of contact between granules. Liquefaction of minute portions of the granules will occur at the points of greatest compression; and, moving to points of less compression, this moisture becomes attached there by crystalline force. Actual melting, however, is not necessary. Slower transfer of molecule by molecule (idiomolecular transfer) accomplishes the result. As these transfers are in response to stress, the result is easing of the stress by yielding. Some granules lose and others gain, and their positions with respect to one another change somewhat. The summation of slight adjustments between innumerable granules is general movement of the mass in the lines of least resistance.

Associated with this granular motion there appears to be movement along shearing planes. Attempts to measure instrumentally this shearing were made on glaciers in the Canadian Rockies in 1910, in Alaska in 1919, and in the Alps in 1921, by Dr. R. T. Chamberlin,

[1] *Decennial Publications of the University of Chicago*, series 1, IX (1904), 191–206.

[2] *Journal of Geology*, XV (1907), 189.

[3] Rollin T. Chamberlin, "Instrumental Work on the Nature of Glacier Motion," *Journal of Geology*, XXXVI (1928), 19.

using methods and apparatus which he and his father had devised. After some ineffectual trials, results were obtained which seemed to show that "at times there was almost no differential movement of the ice; at times there was a slow, steady shearing of the upper ice over the lower; while at other times there was a rapid slipping of the upper ice over the lower." It was inferred from the observations where the test was made that no great accumulation of differential stress could take place in glacier ice without causing adjustment.[1]

CAUSES OF CONTINENTAL GLACIATION

Prior to the late 1890's one finds but little in T. C. Chamberlin's writings treating of the causes of continental glaciation. As the field mapping and study of the Pleistocene glacial deposits progressed in Europe and America and the evidence of diversity as opposed to unity of the glacial period accumulated, it became more and more evident that the current explanations as to the causes of continental glaciation did not meet the demands of the situation. Neither the postulated high elevations nor the planetary relations appealed to in Croll's and other astronomical hypotheses seemed competent to explain the very notable succession of alternating epochs of glaciation and deglaciation which were found to have taken place in Pleistocene time. These matters were being discussed at the University of Chicago and Chamberlin, with the co-operation of his colleagues and advanced students, began the work of building up a hypothesis in the hope of developing something more satisfactory. As the studies progressed the results were published in a series of articles in the *Journal of Geology*. A brief outline of the theory appeared in November, 1897, under the title, "A Group of Hypotheses Bearing on Climatic Changes." This had been presented August 20, 1897, before the Toronto meeting of the British Association for the Advancement of Science. Omitting the discussion of the conditions of enrichment of the atmosphere, certain excerpts may be taken from this article and from an article in Volume VI of the *Journal of Geology* entitled, "The Influence of Great Epochs of Limestone Formation upon the Constitution of the Atmosphere," as bearing on the depletion of the CO_2 of the atmosphere and its effects on temperature:

[1] *Op. cit.*, p. 21.

Carbon dioxide, though quantitatively a minor component of the atmosphere, is necessarily a critical factor in vital processes and in many of the most important of inorganic processes. It is regarded, e.g., as "a leading agency in the disintegration of crystalline rock."

Chamberlin states:[1]

. . . . If a computation be made of the amount of carbon dioxide that would be required to disintegrate the crystalline rock requisite to supply the clastic material for a great epoch of sandstone and shale deposition (allowing duly for old clastics used over), a competency to exhaust many atmospheric equivalents of carbonic acid will be shown. This, being correlated with limited limestone formation, and consequent scant returns of carbonic acid from the ocean, seems competent on its side to notably change the constitution of the atmosphere in the direction of poverty of carbonic acid. . . . .

Let us assume for the moment a uniform supply equal to the *average* rate of exhaustion. With the inauguration of any great epoch of general uplift there would begin an era of relatively rapid atmosphere exhaustion, which would proceed continuously during such elevated stage and might result in notable atmospheric impoverishment, as the computations cited early in this paper show. As the cutting down of the surface approached baselevel, the depletion would be retarded and, the supply continuing the same by hypothesis, the rate of exhaustion would fall below that of supply and an epoch of enrichment begin. A second elevation would re-inaugurate the depletion, and so oscillations of enrichment and impoverishment would follow the general oscillations of the land surface. Applying this law by itself, atmospheric poverty should follow *at some distance* the stages of general elevation, and, on the other hand, atmospheric enrichment should follow at some distance the stages of baseleveling or depression. . . . .

It is impossible here to attempt to apply the doctrine in detail to geological history. But it may be noted in passing that the Pleistocene glaciation followed at a notable interval the formation of the great plateaus and epeirogenic uplifts of late Tertiary times. The glaciation of India, Australia, and South Africa occurred about the time of the crustal revolutions that marked the close of the Paleozoic era. The uncertainty of the homotaxis of the strata involved makes a precise correlation at present impossible. The glaciation perhaps came too early to fit the hypothesis. Here, at least, is an excellent chance to put it to trial. All other hypotheses of glaciation have fared badly when brought to the supremely severe test of the ancient oriental low-latitude glaciation, and if this hypothesis shall follow them to the junk shop of broken down theories it will find an already beaten path. . . . .

It is now a little more than fifty years since Tyndall suggested that the periods of terrestrial glaciation might be dependent upon the carbon dioxide of the atmosphere whose peculiar competence to retain solar heat he had demonstrated. The suggestion of the origin of glaciation through the depletion of this

[1] *Journal of Geology*, VI, 620; V, 678 and 680–82.

atmospheric constituent is, therefore, not at all new. It has been entertained by others than Tyndall. If it has failed to find much acceptance this has perhaps been partly from a doubt as to its adequacy and partly from the lack of any definitely assignable cause for the requisite intermittent depletion. Dr. Arrhenius has recently contributed to the subject a most important discussion bearing especially upon the former point.[1] By an elaborate mathematical analysis of data derived from Langley's experiments he has endeavored to ascertain what degree of depletion of the carbon dioxide of the present atmosphere would bring on the conditions of Pleistocene glaciation, and, on the other hand, what degree of enrichment would produce the warm climate of the Tertiary. He arrives at the conclusion that the removal of 38 to 45 per cent of the present carbon dioxide would bring on glaciation and that an increase of 2.5 or 3 times its value would produce the mild temperatures of the Tertiary times. He quotes the opinion of Professor Högbom in support of the competency of earth changes to produce this depletion, and also the competency of the interior and other sources to resupply the impoverished atmosphere. He, therefore, carries the suggestion of Tyndall and others a very notable step in advance, and, what is especially important, has given it quantitative expression on the basis of deductions from observed data. He does not, however, postulate the conditions which control the enrichment and depletion of the atmosphere which has been the essential endeavor of this paper.[2]

But we do not meet geological demands when we simply offer *general* explanations of climatic changes. Our theories must ultimately be found to fit the precise phenomena. How are we to explain the profound glacial oscillations? . . . . I have endeavored to follow out the doctrine of atmospheric gain and loss on its own lines, and although the studies are incomplete, the results are at least encouraging. I seem to find a rhythmical action that may in part explain the glacial oscillations. To do it justice it should have elaborate and careful statement, but I can here only suggest its nature in bald outline and in terms that need qualification. The idea hinges (1) on the action of the ocean as a reservoir of carbon dioxide and (2) on the losses of the organic cycle under the influence of cold. Cold water absorbs more carbon dioxide than warm water. As the atmosphere becomes impoverished and the temperature declines, the capacity of the ocean to take up carbon dioxide in solution increases. Instead, therefore, of resupplying the atmosphere in the stress of its impoverishment, the ocean withholds its carbon dioxide to a certain extent, and possibly even turns robber itself by greater absorption, though the diminution of the tension of the carbon dioxide of the atmosphere as its amount is reduced tends to in-

[1] Svente Arrhenius, *Phil. Mag.*, S. 5, XLI, No. 251 (April, 1896), 237–79.

[2] I may here remark that the main features of the ideas herein advanced were entertained and expressed to my students some time before I saw Dr. Arrhenius' important paper, but I fear I might not have felt justified in giving them a more public statement but for the encouragement of weighty opinion on the vital point of quantitative sufficiency.—T. C. C.

crease the discharge of carbon dioxide from the ocean to restore the equilibrium, and, to the degree of its efficiency which is undetermined, offsets the increased absorption of the cold water. So also, with increased cold the process of organic decay becomes less active, a greater part of the vegetal and animal matter remains undecomposed, and its carbon is thereby locked up, and hence the loss of carbon dioxide through the organic cycle is increased. The impoverishment of the atmosphere is thus hastened and the epoch of cold is precipitated.

With the spread of glaciation the main crystalline areas, whose alteration is the chief source of depletion, become covered and frozen, and the abstraction of carbon dioxide by rock alteration is checked. The supply continuing the same, by hypothesis, reenrichment begins, and when it has sufficiently advanced warmth returns. With returning warmth, the ocean gives up its carbon dioxide more freely, the accumulated organic products decay and add their contribution of carbonic acid, and the reenrichment is accelerated and interglacial mildness hastened.

With the reexposure of the crystalline areas, alteration of the rocks is renewed and depletion reestablished and a new cycle inaugurated. And so the process is presumed to continue until a change in the general topographic conditions determines a cessation.

The localization of North American and European glaciation is regarded as due to the great areas of permanent atmospheric depression near Greenland, the Aleutian Islands, and the related storm tracts.

As to periodicity, it is stated[1] in part:

The basal conception is that, under general conditions favorable for glaciation, certain of the agencies involved became dominant and tended to intensify and accelerate glaciation for a time, until they either pushed the effects to an extreme from which a reaction was inevitable, or they exhausted themselves temporarily, while other agencies of opposite phase, which had been subordinate until then, became dominant and forced a reaction.

When a reaction was set up, it in like manner was pushed to an extreme, and deglaciation extended beyond the point of equilibrium for the average conditions. And so oscillations beyond and short of the mean state, gave a rhythmical phase to the glaciation of the period. The rhythm, we learn from observation, took the form of a series of sub-equal oscillations with declining time-intervals. There seem to have been no great differences in the amplitude of the ice advances. Observation does not permit us to speak as confidently of the extents of the recessions. It is important to note that the fundamental or general conditions remained effective throughout the period, and that the oscillations are regarded only as rhythms superposed on these general conditions. The more intense phases of these rhythms were, however, the only portions of the series that recorded themselves in glaciation near the borders of the glaciated

[1] Chamberlin and Salisbury, *Geology*, III (1906), 433 and 436.

areas, and were perhaps the only portions that recorded themselves in continental glaciations at all. The retrocessional phases may have been recorded only in cool climates in high latitudes, and in glaciation at high altitudes.

LATER CONNECTIONS WITH FEDERAL AND STATE SURVEYS

In the late 1890's and in the 1900's, Professor Chamberlin turned more and more to the broader fields of geology and cosmogony, with which this sketch has not to deal, and as time went on he wrote fewer papers on glacial geology. He continued his supervision of the glacial investigations for the Federal Survey, however, for many years and was frequently in consultation with members of state surveys. Among those of whose glacial studies he had supervision for periods of different lengths were: R. D. Salisbury, F. B. Taylor, W. G. Tight, James H. Smith, J. W. Goldthwait, and T. O. Mabry. He had also close connection with studies made in the West for the Federal Survey, under the immediate direction of R. D. Salisbury, by W. W. Atwood, G. H. Garrey, Eliot Blackwelder, F. H. H. Calhoun, S. R. Capps, and others. The present writer counts it a great privilege to have worked for many years in southeastern Wisconsin, and for shorter periods in Illinois, Pennsylvania, and Massachusetts, under the immediate supervision of Dr. Chamberlin. Mr. Leverett's association with him began about ten years earlier and continued for a longer time. These various studies were in widely scattered fields in many different states and from these has come a long series of official and other papers bearing the direct impress of Dr. Chamberlin's masterly mind. Of these perhaps the most notable are the monographs by Mr. Leverett; besides those cited above, Leverett's official reports include the following: *Monograph 41*, "Glacial Formations and Drainage Features of the Erie and Ohio Basins; *Monograph 53*, "The Pleistocene of Indiana and Michigan and the History of the Great Lakes" (with F. B. Taylor); *Professional Paper 154*, "Moraines of the Lake Superior Basin" (in press); *Professional Paper* ——, "Quaternary Geology of Minnesota" (in preparation).

Even after Professor Chamberlin had retired from active connection with the Federal Survey and with the University of Chicago, glacial geologists of this and other countries sought his counsel. His direct influence in glacial geology was long continued and by no

means circumscribed by the limits of his fatherland. Some of the latest of his unpublished writing was in regard to the classification of the Pleistocene of the Mississippi Valley.

In closing this sketch concerning the contributions of Thomas Chrowder Chamberlin to glacial geology, the writer cannot do better than to call to mind that "restraint, patience, and equipoise" which quite generally characterized his treatment of students and colleagues even under the occasional stress of great provocation. In the old days at the University of Chicago, and ever afterward, his students spoke affectionately of him as a kindly gentleman, a great scholar, and an inspiring teacher.

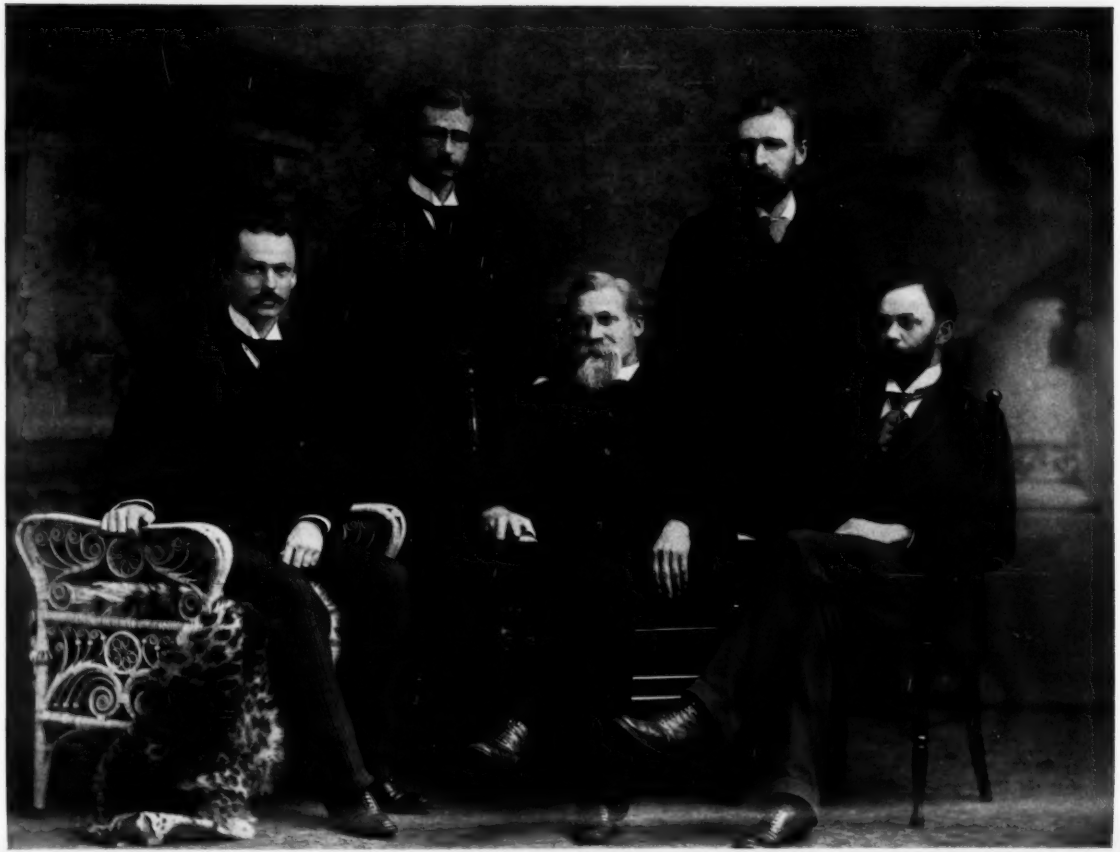
THE EARLY DAYS OF THE DEPARTMENT OF GEOLOGY AT THE UNIVERSITY OF CHICAGO

R. A. F. PENROSE, JR.
Philadelphia

THE writer has been asked, as the surviving member of the resident group composing the first Faculty of the Department of Geology at the University of Chicago, to write a brief sketch of its early history as developed under the influence of T. C. Chamberlin. Others have spoken eloquently in this Memorial Number of his scientific and philosophic accomplishments in both the study of terrestrial geology and in his interpretation of the phenomena of his planetesimal hypothesis. The writer therefore will confine his remarks to the wonderful ability with which he organized his Department and the *Journal of Geology*, and the efficiency in teaching and research with which he inspired all who came in contact with him, and which will long remain a forceful and beneficent influence in the years to come.

The charter of the new University of Chicago was dated September 11, 1890; William R. Harper was appointed President, and he assumed the duties of his office on July 1, 1891. T. C. Chamberlin was promptly appointed Professor of Geology and Head of his Department, which together with other departments, threw open its doors for students on October 1, 1892. Thus within about two years after the charter was granted, a man of wonderful learning and executive ability, a natural leader of men, had been chosen President, and he in turn had established different departments, had thought out the splendid grouping of University buildings, and had started a center of high learning, which arose as if by magic from former prairie land west of Lake Michigan.

One of the first full programs of courses at the new University was that of the Department of Geology announced in October, 1892, and the first faculty consisted of: T. C. Chamberlin, Professor of Geology and Head of the Department; R. D. Salisbury, Professor of Geographic Geology; J. P. Iddings, Associate Professor of Pe-



R. A. C. Perkins, Jr. J. E. Johnson L. C. Chamberlain C. R. Van Hise R. D. Salisbury

trology; R. A. F. Penrose, Jr., Associate Professor of Economic Geology; C. R. Van Hise, non-resident Professor of Pre-Cambrian Geology; W. H. Holmes, non-resident Professor of Archaeologic Geology; George Bauer, of the Biological Department of the University, Assistant Professor of Paleontology; Edmund Jüssen, Docent in European Stratigraphy.

The first five of this Faculty functioned actively in the term of 1892 to 1893. In 1894 Charles D. Walcott was appointed non-resident Professor of Paleontologic Geology, and E. C. Quereau was appointed Docent in Paleontologic Geology. In 1895 J. P. Iddings and R. A. F. Penrose, Jr., were promoted from Associate Professors to Professors in their particular subjects; in 1896 Stuart Weller was appointed Assistant in Paleontologic Geology, and O. C. Farrington was appointed Professorial Lecturer in Determinative Mineralogy, while the title of W. H. Holmes was changed in the departmental announcement of 1894 to Professor of Archaeologic and Graphic Geology.

The Department of Geology never went through the initial struggle for existence and recognition that has characterized scientific departments in many universities, but it started even in its very first months as a wonderfully conceived and efficient institution. Chamberlin had resigned as president of the University of Wisconsin, where his ability as an organizer and administrator had built up that institution to one of world-wide recognition, and had accepted the position of Professor of Geology and Head of the Department at the University of Chicago. He left Wisconsin with the deep regret of those interested in that institution, and was welcomed at the new University as an inspiring genius at a time when such a man was needed. He had made the change only after careful consideration and with a feeling of reluctance to leave the field of his great accomplishment in Wisconsin, but he desired to be more free of administrative work and to devote himself more closely to his chosen science; and though the Department of Geology at Chicago required the benefit of his wide experience in administration, yet he was able to a greater extent than formerly to devote himself to teaching and to research.

In the same way Salisbury, who was professor of geology at the

University of Wisconsin under Chamberlin, was induced to leave that institution only because his friend and chief urged him to do so. Iddings and Penrose were also offered positions on the faculty without any request on their part, while Van Hise, who was then professor of Pre-Cambrian geology at the University of Wisconsin, agreed to give a six weeks' course of lectures at the University of Chicago. In a like manner W. H. Holmes, of the Smithsonian Institution at Washington, consented to give lectures at intervals on archaeologic geology. It will thus be seen that the first geological faculty of the University was composed of men who were asked to join it, and not of men who sought the positions. The writer mentions this fact because in later years the development of the Department was undoubtedly attributable to the spirit of originality and efficiency which comes from men who accepted appointments because they offered unusual opportunities for study and research.

Chamberlin proceeded immediately to organize his Department with that same wisdom and vision of the future which had in previous years built up the University of Wisconsin, and it opened with an unusually large attendance of students. The construction of University buildings for teaching, museums, and other purposes, had as yet not much more than just begun, and most of those that were finished were required for the executive offices and for dormitories. The Department of Geology therefore was temporarily housed in two small brick buildings on the south side of Fifty-seventh Street, close to the University grounds. Here was seen in those early days an active faculty led by their presiding chief, and a group of earnest, intelligent students who filled the rooms of the small brick buildings to repletion; here the spark of inspiration started by Chamberlin aroused the enthusiasm of both the geological Faculty and the students; here was taught the importance of geology not only as a cultural study, but as an incentive to research and other pursuits, and here were worked out the plans for the publication of the *Journal of Geology* to be the means of intercommunication with the outside world.

In the first announcement of the Department of Geology the statement was made:

> The Department has been organized with a view of providing systematic training in geology (embracing as constituent sciences, Geography, Mineralogy

and Petrology), in such a form as to be serviceable as a part of a liberal education and, at the same time, to be specifically preparatory to professional and investigative work in the science, either in connection with educational institutions, official surveys, industrial enterprises, or private researches. The first purpose predominates in the earlier courses and the second in the later, but both have a place in all and find their realization in a common method of treatment.

This comprehensive conception of the purposes and objects of the new Department, as announced by Chamberlin in his first program, displayed his liberal grasp of the many important features of a study of geology. The history of the Department has shown how splendidly his broad vision grasped the possibilities of the new institution.

The announcement of the coming publication of the *Journal of Geology* was also made at the same time, as follows:

A Department journal will be issued, which will not only serve as a medium of publication for the results of researches connected with the Department, but will bring to the University and to its constituency, some of the products of leading geologists in this and other countries. It will not, it is hoped, be simply a means of communication between the University and the public, but of intercommunication between investigators within and without the University. . . . . It will not largely be the medium of publishing local details, or merely facticious (sic) descriptions, except as these may have obvious bearings upon questions of wide interest or fundamental importance, or may have immediate relations to the environment of the University.

Here again in the conception of the field of activity of the *Journal of Geology*, Chamberlin displayed his remarkable initiative and breadth of vision in planning a publication which would be devoted not only to the contributions of the Department, but to similar contributions from leading geologists throughout the world; and more than all, that the *Journal* would not be a medium for publishing simply local descriptions, but that it would be developed in such a way as to bear mostly upon broad conceptions of problems of fundamental importance. No geological journal, if indeed any journal in any branch of science, was ever designed with a grander ideal than that displayed in the foregoing announcement; and the history of the *Journal of Geology* since its foundation over thirty-six years ago has maintained this spirit and has commanded the respect not only of all geologists but also of scientists in general throughout the world.

The ambition of President Harper was to establish various jour-

nals in different departments of the University, and in 1892 the *Journal of Political Economy* was the first to make its appearance. In the meanwhile the *Journal of Geology* had been conceived and the plans for its publication had been made, so that shortly after the *Journal of Political Economy* had started the *Journal of Geology* made its appearance as the second of the Departmental journals, at the beginning of the year 1893. The first editorial staff of the *Journal of Geology* was composed of T. C. Chamberlin, R. D. Salisbury, J. P. Iddings, R. A. F. Penrose, Jr., C. R. Van Hise, C. D. Walcott, W. H. Holmes, and various associate editors chosen to represent different institutions of learning in the United States, Europe, and South America.

Thus within a year from the opening of the University the Department of Geology had not only been developed to a high degree of efficiency, but its medium of reciprocal communication with the outside world, the *Journal of Geology*, had become a reality. Both the Faculty and the *Journal* combined to create that spirit of efficiency, sympathetic understanding, and idealism which molded them into an active agent in the cause of science. Chamberlin was in constant co-operation with President Harper, who relied much on his broad knowledge and his experience in university administration, and thus that sentiment of good will between scientific Faculties and the University at large which was essential to great accomplishments was still further strengthened. In the early years of the University the general Faculty met in the south wing of Cobb Hall, where were also located the office of the President and other administrative bureaus. The writer, who was a member of the Faculty at that time, recalls with vivid admiration the commanding position which was at once assumed by Chamberlin in that organization and which was accepted by its members, not with any jealousy or dispute, but with a feeling of gratitude that such a man was among them.

In 1893 the Department of Geology moved into the recently erected Walker Museum. Chamberlin had been Director of Museums almost from the beginning of the University, and he was thoroughly familiar with the ambition of George C. Walker to have the building erected by him used for general museum purposes; but

the collections desirable for a university museum had as yet not accumulated to any great extent, so that Walker, who was a very liberal and broad-minded man, consented to allow the Department of Geology, until they might acquire other quarters, to occupy the building for instruction, research, and the storing of materials that might eventually form part of the geological section of the Museum.

The Department was thus installed in a large new building which, though not constructed primarily for their special uses, answered the purpose so well that it remained the home of the Department of Geology for many years. In 1912 Julius Rosenwald founded the beautiful building which bears his name. It was erected immediately adjacent to Walker Museum and, when finished, became the home of the educational staff of the Department of Geology and Paleontology, while many of the geological collections and exhibits were still retained in Walker Museum in accordance with the wishes of its founder.

In the meantime, however, as far back as 1893, Chamberlin desired to accomplish as far as possible the wishes of George C. Walker eventually to have the Walker building used entirely as a museum, and efforts were made to begin this movement by the establishment of a collection of minerals, paleontological specimens, relief maps of different parts of America and other countries of the world, and such other collections as might be the foundation of at least one of the displays desired by Walker.

The writer was intrusted with the problem of starting a mineralogical collection, and he presented his own somewhat limited personal collection to the University. At that period also the World's Columbian Exposition in Chicago was approaching the time when it would close, and the question with many of the exhibitors had arisen as to what to do with the materials they had assembled there. Many of the states of the United States and many foreign countries had brought to Chicago specimens of their minerals, ores, and other exhibits for display. The writer was successful in securing for the University some valuable gifts for the mineral collection of the Department of Geology, including many contributions from English, English Colonial, French, Swedish, and other foreign exhibits, as well as from the individual collections of many states in America.

A large part of the splendid exhibit of the state of California was secured for the University.

For many years the collection thus started was housed in the eastern end of the ground floor of Walker Museum, in cases and drawers that had been bought for it, and it was labeled by the writer. Since that time, however, the collection has probably been reorganized, but it is an interesting fact to know that the beginning of the mineral collection of the University of Chicago started largely with material collected from the World's Columbian Exposition in 1893. Still more material both of a mineralogical character and of other importance as museum exhibits might have been obtained at the closing of the World's Columbian Exposition had it not been that just at that time the Field Columbian Museum was started by the liberality of Marshall Field of Chicago. Under this organization the Fine Arts Building, one of the most substantial structures of the World's Columbian Exposition, was acquired for the collection of such contributions as the different American and foreign exhibitors might be willing to present. It thus accumulated a vast amount of material—some of great importance and value, some of but little value, but all gathered together where it was in later years carefully sorted, arranged in proper cases, and created into an elaborate general museum.

The Field Columbian Museum occupied the old Fine Arts Building for many years, when finally a better site nearby was given it. With additional liberal contributions from Marshall Field, the Field Columbian Museum, later known as the Field Museum of Natural History, has progressed wonderfully and is now one of the great institutions of its kind in the United States, not only in its displays but in its research work, and in the expeditions carried out under its auspices.

In 1919 Chamberlin resigned as Professor of Geology and Head of the Department and was appointed Professor Emeritus; this gave him the leisure he had long sought to enable him more actively to continue his work in the problems of cosmogony, on which he had for many years been engaged and to which he now could give his entire attention. In thus finally relinquishing active teaching and administration, he must have been deeply gratified to realize that

he left in the geological Faculty a group of men who appreciated his wonderful ability, who had learned to benefit by their contact with him, and whose respect, admiration, and affection followed him in his retirement.

He has now passed away, but his genius, his accomplishments, and his great and remarkable personality, which seemed to grow in grandeur as his knowledge of the vast cosmos grew, will long be a brilliant inspiration to those who will continue to unfold the phenomena and philosophy of nature.

CHAMBERLIN'S PHILOSOPHY OF CORRELATION

CHARLES SCHUCHERT
Yale University

THE writer feels highly honored by being asked to write a chapter in this memorial number to one of America's foremost geologists, the leader not only in studies relating to the origin of the earth, but also in those furthering strikingly the application of diastrophism to our everyday work in stratigraphy. What follows is an appreciation of Chamberlin's presentation of how diastrophism reacts on marine sedimentation and life, its periodic occurrence, and why it is the ultimate basis in the delimitation of at least the periods and eras of geologic time.

Between 1898 and 1918 Chamberlin published some fourteen papers (130 pages) bearing directly on diastrophism, the fundamental conclusions of which will be later summarized, mainly in his own words. At least fifteen years earlier, however, the cyclic nature of the oceanic transgressions over North America was crystallized in his mind, as may be clearly seen in Volume I of the Geological Survey of Wisconsin (1883). Here he sets forth the local phenomena of the Cambrian, Ordovician, Silurian, and Devonian, along with the paleogeography, and concludes: "Ideally there might be said to be three main epochs in each period: (1) that of advancing waters and coarse detrital deposits; (2) that of deep waters and limestone deposits; and (3) that of retiring waters and mixed shaly deposits." This ideal condition, however, is "usually broken by minor oscillations and irregularities of movement" (p. 210).

The idea of the cyclic nature of marine flooding and sedimentation of course goes back at least to John Strong Newberry's epochal paper of 1874 entitled "Circles [or Cycles] of Deposition in American Sedimentary Rocks." As Willis said in his address of 1910 on "Principles of Paleogeography":

> American geologists need no restatement of the phenomena of cycles of deposition and erosion which Newberry emphasized and which have led through the work of Powell, Gilbert and Davis to recognition of the principle that epochs of marked relief and vigorous erosion have alternated with periods of base-

leveling, and that sediments have alternated correspondingly in character and volume [p. 248].

And Chamberlin himself in his American Association address, "Seventy-five Years of American Geology" (1924), states:

It is historically worth while to note that alternations of relative quiescence and disturbance and the rhythmical play of erosion and deposition were early recognized and emphasized.

In the same way Eduard Suess acknowledged the value of Newberry's short paper in his discussion of the cyclic phenomena in geology in the first two volumes of his great work, "Das Antlitz der Erde," appearing in 1885 and 1888. Here Suess traces descriptively the marine invasions over the lands, and especially those of the Middle Cretaceous and Middle Devonian, classing them as "continuous, eustatic positive movements of the strand-line," and saying that "the theory of secular oscillations of the continents is not competent to explain the repeated inundation and emergence of the land. The changes are much too extensive and too uniform to have been caused by movements of the earth's crust." What Suess did so well along descriptive lines, Chamberlin did even better along theoretic ones. It should be noted, moreover, that both independently came to the same major conclusion, namely, that it is not crustal movement that brings on the maximum of continental flooding.

Now let us take up in more detail Chamberlin's various conclusions on diastrophism. His first paper on the subject, "The Ulterior Basis of Time Divisions and the Classification of Geologic History" (1898), may be regarded as an introduction to the series. In the preceding number of the *Journal of Geology* (May–June, 1898) there was a symposium on the classification and nomenclature of geologic time divisions, which had to do with the results of a questionnaire sent out by Professor Salisbury. This was concerned mainly with the terminology of geologic time, but the question was also asked: "Upon what criteria shall the divisions be determined?" Curiously no one in the symposium took up this vital question; but Chamberlin later on did. In the paper cited above he says:

The most vital problem before the general geologist today is the question whether the earth's history is naturally divided into periodic phases of world-

wide prevalence, or whether it is but an aggregation of local events dependent upon local conditions uncontrolled by overmastering agencies of universal dominance [p. 449].

He points out that there are of course no universal breaks in sedimentation or in the evolution of life. Accordingly, there are no *absolute* rock or time divisions. But this conclusion

does not dismiss the question whether this continuity of physical and vital action proceeded by heterogeneous impulses or by correlated pulsations. If the latter, then the history of the earth, when deciphered, will assume a rhythmical periodicity susceptible of natural classification and of significant and rational nomenclature [p. 450].

Chamberlin maintains that there is such a rhythmical periodicity, and argues it along three general grounds. Only the first two, however, need be here considered, since the third, relating to the carbon dioxide theory, has not stood the test of time. In the first place, "great earth movements affect all quarters of the globe . . . . leading to general readjustment. . . . . The oceanic basins become progressively deeper and more capacious, while the continents become higher [degradation aside]." These conclusions, he is well aware, cannot be fully demonstrated until much more of the earth has been studied. Secondly, "the major movements of the earth's surface have consisted of the sinking of the ocean bottoms and the withdrawal of additional waters into the basins whose capacities were thereby increased." These movements are thought to be explainable by another postulate, namely, that the earth shrinks periodically, and that "the radial shrinkage of the ocean bottoms has surpassed the radial shrinkage of the continental platforms to the average amount of some 10,000 or 12,000 feet." In addition, the oceans have also grown larger in area. This increased capacity is due to crustal readjustments brought about by the internal changes, whereas the external ones "work to precisely opposite ends, the degradation of the land and the filling of the basins." The increase in oceanic capacity, moreover, is attested in the "circumcontinental terrace." In other words, the present continental shelf is such a circumcontinental terrace and its "extreme limit of development is attained when the continent has been base-leveled and no farther detritus is furnished for the extension of the

terrace." The circumcontinental terraces and their entombed life of the past are the main records accessible to geologists and paleontologists. "Such a succession of shallow sea incursions and withdrawals reciprocating with crustal movements and quiescence seem to me well indicated as the master features of geologic progress from the beginning of the Paleozoic era to the present time" [pp. 450–59].

In a second paper, "A Systematic Source of Evolution of Provincial Faunas" (1898), he says that periodic diastrophism is evidenced by "the great base-levels and the great epochs of mountain making, the former pointing to long periods of relative quiescence, the latter to exceptional periods of disturbance" [p. 599]. The warping of the continental platforms is partly due

> to the lateral thrust of segments of the earth as they sink [especially those segments that lie beneath the ocean], partly to internal changes of temperature and the intrusion of liquid matter, and partly to the settling of the continent when, by any of the preceding agencies it has been forced upward beyond the plane of isostatic equilibrium, the settling being accomplished through the slow quasi-fluid creep of the rock under gravitative stress [p. 600].
>
> The deposit of the detritus of the land in the sea raises its level. If the average elevation of the present land be taken at Lapparent's figures, 2,120 feet, its truncation and removal to the ocean would lift the sea-level 700 feet [making no allowance for the spread of the sea] [p. 602].

The overlaps of the oceans on to the continents he here names *epicontinental seas*.

In 1909 appeared "Diastrophism as the Ultimate Basis of Correlation." If body deformations are "heterogeneous and the combined results irregular and uncertain," Chamberlin does not see how they can be made a very trustworthy basis of systematic correlation. But if the deformations are inheritances and the succession homogeneous, then the results co-ordinate. "Under this view, ocean basins and continental elevations tended toward self-perpetuation"; and this is the view accepted by Chamberlin "with much confidence."

The great base-levelings and sea-transgressions "are little more than alternative expressions for the same thing," namely, periodicity in crustal stability and world-warping deformations. "Correlation by base-levels is one of the triumphs of American geology," and to it we may now add correlation by its complement of sea-

transgressions. On the other hand, "in a quiescent state the base-leveling of the land means contemporaneous filling of the sea-basins by transferred matter, and hence a slowly advancing sea-edge which is thus brought into active function as a base-leveling agent. This water movement is essentially contemporaneous the world over, and is thus a basis for correlation" [pp. 689–90].

"Correlation by general diastrophic movements takes cognizance of four stages: (1) the stages of climacteric base-leveling and sea-transgression, (2) the stages of retreat which are the first stages of diastrophic movement after the quiescent period, (3) the stages of climacteric diastrophism and of greatest sea-retreat, and (4) the stages of early quiescence, progressive degradation, and sea-advance" [p. 691]. Stratigraphy and paleontology have been the main reliance in correlation, but "diastrophism lies back of both and furnishes the conditions on which they depend" [p. 692]. Accordingly diastrophism is the ultimate basis of correlation.

Beginning with 1913 Chamberlin again took up the meaning of crustal movement in relation to stratigraphy under the general title "Diastrophism and the Formative Processes." In this series there are nine papers, one being by his son Rollin (Part VII). In Part I, "Introduction," Chamberlin postulates an elastico-solid, crystalline earth, favoring the idea that the earth had been in this condition since an early stage of growth if not from the very beginning.

Part II, entitled "Shelf-Seas and Certain Limitations of Diastrophism," also appeared in another form as "The Shelf-Seas of the Paleozoic and Their Relations to Diastrophism and Time Divisions." The latter was presented before the Toronto meeting of the Twelfth International Geological Congress and is printed in the 1914 *Compte Rendu*. The following quotations are from both of these papers. Here he distinguishes between basins due to deformative causes (=diastrophic basins) and gradational basins. Of the latter the most familiar ones of the present are the shelf-seas, but these also have their beginnings in deformations. Older ones are the epicontinental seas but not all of these are gradational seas. "It seems safe to say that 80 or 90 per cent of the surface areas of the continents bear some evidence of former shelf-sea work and 70 per cent or more

are still mantled by the shelf products. . . . . Shelf-sea work is pre-eminently a process of terracing. . . . . The picture of the continents as essentially terraces wrought upon diastrophic embossments is no doubt the truest that can be formed" [1913, pp. 527–28].

The ordinary diastrophism of the earth springing from internal stresses is, "in its very nature, unsuited to adjust the surface of the earth to the surface of the sea over wide areas with such close nicety as effectively to facilitate the great sea-transgressions and the formation of great terranes of marine sediments that spring from them" [Part III, p. 577]. Nor are they due "directly to vertical or epeirogenic movements of the earth's crust" [1914, p. 545]. It appears that diastrophism is "far from being a cooperative agency fitted to promote broad, regular, serial stratification, such as distinguishes the mid-stages of the periods of the Paleozoic" [p. 547].

The present recognized systems in our geological time-table are not fully expressive of diastrophism, "for these systems are not wholly satisfactory. There is reason to think that an ideal criterion would modify these systems in some important respects" [p. 551].

"The criteria that best mark the beginning and ending of historic periods" lie in the major deformative movements, which "are fundamental to both stratigraphic procedure and life evolution." On the other hand, the periodic movements may "vary widely in the intensity of their effects in different quarters of the globe without nullifying the universal periodicity of the shelf-sea work and all that goes with it" [p. 552].

"From the philosophical point of view, the beginning of an important diastrophic movement is entitled to be regarded as the initiation of a new period, since its normal sequences are a new attitude of the sea-level, a new régime of erosion and deposition and a new phase of biologic evolution. But, at the outset, this is prophetic rather than realized." It "lacks availability as a practical way-mark." But when the diastrophic cycle is completed, it is "withal less subject to differences of interpretation and determination than are most of the signs of a great change in the course of biologic evolution." Because diastrophism alters the environment and often fundamentally so, each major crustal movement brings on "new phases of evolution in most or all of the leading life types at the same time.

. . . . Beyond question, such a general change is one of the most obvious credentials of a new period. But if it is a result rather than a cause, and if the cause has well defined characters of its own available as landmarks, the fundamental seems to me entitled to precedence over the sequential. . . . . The change of the environment that induced the new events offers, in my judgment, a sharper and better landmark in most cases" [pp. 552–53].

Diastrophism is undoubtedly the basic principle on which the geologic time-table will be established, yet the writer has long held that it alone can never fix the limits of the periods and eras. The times of crustal deformation are recorded not only in angular unconformities, but in erosional ones as well, and more numerously in the disconformities; and which of these is of especial period or era value can be determined only by the entombed fossils. Diastrophism and fossils in correlation therefore work to the same end.

Part III has to do with "The Lateral Stresses within the Continental Protuberances and Their Relations to Continental Creep and Sea-transgression." Here Chamberlin holds that such vertical stresses as may arise from loading and unloading in the process of gradation are also unsuited to produce the epicontinental seas, "because loading and unloading tend to produce a warp between the loaded and the unloaded tracts, whereas great sea-transgressions and great terranes of parallel sediments require either a movement that retains the parallelism of the sea-surface and the sea-bottom, or else the essential absence of any movement at all. A constant upwarping of the land inevitably defeats extensive sea-transgression and its consequent terranes" [pp. 577–78].

"Ordinary diastrophism" embraces the orogenic and epeirogenic movements, while the sequent diastrophism has to do with "the unbalanced stresses that inevitably arise *within* the continents as a consequence of their own protrusions. . . . . These stresses depend simply on gravity acting on the protruding matter as such without regard to other conditions" [p. 578]. This gravitative diastrophism, resulting mainly in horizontal continental creep, is glacier-like, spreading the lands outwardly into the realms of the oceans. "It is only when ordinary diastrophism is quiescent and the continents are in their relatively static stages that the slow, gentle reactionary

movement of creep is presumed to be appreciable" [p. 584]. Accordingly continental creep "is fitted to become a copartner with gradation in leveling the land and thus facilitating an advance of the sea. . . . . The outward creep of the continents reduces the capacity of the ocean basins and thus aids in lifting the sea-level and forcing the waters to creep out upon the lowered land." It is this creeping that appears to bring about the minor oscillations, advances and retreats of the sea, shiftings of outlines, and changes of barriers and connections, "the effect of which is to introduce special features in the sedimentation and local or regional variations in the faunas" [p. 586].

Part IV, "Rejuvenation of the Continents," implies that apparent elevation "was not a continuous steadily progressive process but that it recurred at such wide intervals as to permit great advances toward mature base-leveling as well as the evolution of shelf-seas, of parallel terranes, and of cosmopolitan faunas. All this seems in turn to imply an elastic, rigid, crystalline earth."

Part V, "The Testimony of the Deep-sea Deposits," says: "There must have been pressure contests along the borders [of the continents], and the dividing lines may well have shifted more or less. Growth and creep seaward from the continents is assumed as probable, and periodic counterthrusts landward from the ocean basins are assumed as more than probable. Advances and recessions on the border lines and oscillations up and down are thus of the nature of the case" [p. 139].

In the areas of the hinge-line between the continents and oceans large blocks have gone into the abysses or parts of the latter have risen into mountain folds. The regions where two continents and two oceans come together, such as "the angles of the two Americas and of the Atlantic and Pacific basins," Chamberlin calls the "four-corners of the earth's segmentation." A different kind of very mobile hinge area is that of the Mediterranean. It is in these hinge areas that the most remarkable reversal and interchange between the abyss and the continents occur. But even so, the known areas of abyssal deposits now above sea-level "do not, when all are put together, appear to attain in area so much as one per cent of the total earth surface" [p. 140]. The notably unstable hinge areas have long

been such, and accordingly they do not "affect seriously the broad conclusion of permanency of the true continents and ocean basins" [p. 141].

Chamberlin does not believe in lost continents like that of Gondwana. "I am able to find no evidence of lost continents," he says in 1909, and such vast subsidences "must necessarily have drawn into the cavity . . . . a great volume of the oceanic waters, and, if there was no reciprocal elevation of the ocean bottom elsewhere . . . . there must have been a lowering of the sea-level about all the continents, with a profound effect on the shelf sea-work." Vast changes in the strand line such as are implied by the sinking of a continent have not, he says, been found in the stratigraphic record. Nor do the present sea-bottoms show such submerged plateaus. Chamberlin then goes on to say, however:

> There are well-known ridges on the bottoms of the oceans and others of minor order will not unlikely yet be disclosed. . . . . These may have once been more bowed, or less bowed, than now, and . . . . they may once have cut the sea surface and constituted linear islands, or land bridges, and have played their part in the migrations of plants and animals, just as present bridges may have been once submerged. And these conservative deformations, together with the oscillations and displacements of the segment borders, seem to be about the limit of probable interchange between the real continents and the real ocean basins. Their dominant feature was, as Dana, Wallace, and others long ago urged, permanency [Part V, pp. 143–44].

Part VI has to do with "Foreset Beds and Slope Deposits." This discusses all marine sediments broadly under the heads of (1) topset beds of the shelf-seas, (2) foreset beds of the continental slopes, and (3) flotation beds or deep-sea deposits.

Part VIII is entitled "The Quantitative Element in Circumcontinental Growth." It is very speculative and appears to have at present little direct application in stratigraphy. Part IX, "A Specific Mode of Self-promotion of Periodic Diastrophism," deals with acceleration in deformation. Each general diastrophic movement "brings into play an easy and prompt shifting of load of a special type [epicontinental waters are shifted to ocean basins] that tends to accelerate the primary movement and give it a cumulative value. This tends to push the movement to a maximum the better to prepare the way for a new quiescent period" [pp. 196–97].

Having presented the physical aspects of diastrophism in relation to stratigraphy, we will now take up Chamberlin's views as to how the changed environment reacts on the life of the sea. These are found mainly in "A Systematic Source of Evolution of Provincial Faunas" (1898), which is one of his best pieces of paleontologic work.

The conditions favorable to an expansional evolution of the marine shallow-water life "are those which ensued upon a protracted period of base-leveling. . . . . At the climax of such a period there is normally an extensive transgression of the sea upon the continental platform," and then the sea-shelves are widest and the sea transgresses farthest inland. It is at these times that expansional evolution of marine life is most marked. "A further incident of such stages is the free intercommunication of the life," leading to cosmopolitan faunas.

Restrictional evolution of marine life occurs during the epochs of "profound readjustment to the earth's accumulated contractional stresses." At these times the oceanic basins sink, "withdraw the waters from the surface of the continental platforms," and "establish a new shoreline somewhere near the edge of the continental platforms" or at times and places even farther out on the continental slopes. Accordingly the shallow-water areas are greatly restricted and otherwise physically altered, and while they are shrinking, great quantities of life must die out in the enhanced struggle for existence. "A stage of general repressional evolution is thereby inaugurated and, in a comparatively short period, it is safe to assume, all or nearly all preceding species will have passed out of existence and new species, in a much more limited number . . . . will have been introduced."

Evolution of provincial faunas takes place when the shallow-water seas are restricted to a mere ribbon along the continental shelves. These narrow areas "will be critical and every warping of the surface platform will be decisive either in emphasizing the restrictional influence or in relieving it" [p. 605]. Embayments will appear and in each one there will be more or less development of provincial faunas. "If now a period of quiescence ensues and systematic continental evolution proceeds, these embayments will be-

come extended landward and grow into extensive epicontinental seas, and their faunas will expand accordingly" [p. 606].

When the seas are withdrawn into the ocean basins there probably are always some embayments left which become refuges for the retreating faunas. "Such embayments may be regarded as adventitious" and break "the force of repressional evolution."

Chamberlin's brilliant elucidation of how the periodic crustal movements react on the seas and oceans and how this reaction is recorded automatically in the sediments and entombed organisms has just been presented. Now the question arises, Have these principles become everyday working ones among the students of historical geology? In Europe, very little direct use indeed is as yet made of them, but the students of Mesozoic stratigraphy chiefly, and here and there an orographer, somehow attain results in keeping with Chamberlin's principles. In America the story is quite different, and here Willis, Weller, Ulrich, Bassler, Raymond, Schuchert, and others have been using to good advantage the principles of diastrophism, and through their many students this knowledge has grown into wide acceptance. In other words, of all of Chamberlin's teachings, no other is more widely used than his philosophy of correlation.

The cyclic phenomena in the evolution of the earth are now nearly everywhere in Europe and America an accepted fact in geology, and the various writings of Chamberlin setting forth the manifestations of diastrophism have made the working of this principle all the plainer. In this connection, it may be said that there is no branch of geology that brings out this cyclic nature more plainly than a sequence of paleogeographic maps. The writer was introduced to the wide nature of the cyclic phenomena in 1894 by reading the two first volumes of Suess' *Antlitz der Erde*. In 1903 he began the making of paleogeographic maps, and more especially since 1910 he has been using, as the basis for the delimitation of periods and eras, the cyclic phenomena of diastrophism as defined by Chamberlin, plus the results of organic evolution. But as time goes on and our knowledge becomes more detailed and exact, it is also being made plainer that our accepted periods are not all in harmony with the cyclic truism, partly because there are minor

cycles within the major diastrophic ones, not all of which appear to be explainable by continental creep and by water displacement due to the accumulating sediments. Some of the minor cycles are surely produced by body deformations, either of the continents or of the oceans; and some are of such wide transgression as to call for oceanic subsidence and a changed sea-level amounting to some hundreds of feet (climatic control was absent at these times). On the other hand, it now also appears that the withdrawal of the oceanic waters from the continents at the close of the period cycles is not always as complete as the writer formerly held. Some continents appear to show stratigraphic records completely bridging the supposed breaks between certain periods, making it necessary to be very cautious in deciding just where the line of separation shall be drawn. This point will always be difficult to adjust because of the leanings of the workers, and here as in biology we shall have our "lumpers" and "splitters," but eventually the consensus of opinion will at least adjust the boundary lines. The breaks between the eras, however, still all appear to be present on a grand scale, and some may be of extraordinary duration, though none longer than the one between the Proterozoic and the Paleozoic (Lipalian time).

The Cambrian, Ordovician, and Cretaceous "periods," as now delimited, are surely not in harmony with the teachings of diastrophism, and the Silurian, Devonian, Mississippian, Permian, and Triassic appear to manifest minor diastrophic cycles in their marine oscillations in this or that continent. Chamberlin, however, always recognized and gave considerable thought to the minor diastrophic manifestations superposed on the larger diastrophic cycles. Summing up the conclusions on diastrophism, we cannot do better than to quote the words of Willis (1910):

> Diastrophism has been periodic. Viewed according to the periodicity of diastrophism, the earth's history falls into cycles, and each cycle into [at least] two periods, one of inactivity and another of activity. The periods of inactivity have been long, and during a major part of the duration of any such period the condition of inactivity has been common to the entire surface of the globe. . . . .
>
> The periods of diastrophic activity have been relatively short, and as regards the whole surface of the earth in general not contemporaneous. The great ocean basins are distinct dynamic provinces, and each has experienced periods of diastrophic activity peculiar to its individual history. Orogenic dis-

tricts are sharply limited by local mechanical conditions. . . . . And folding and unconformity, therefore, are frequently not contemporaneous even in one and the same dynamic province. . . . .

The criteria of correlation are both physical and organic. The physical facts are basal. The organic forms, though endowed with evolutionary energy, are dependent and sequential. Any ultimate classification of the earth's history must be founded [however] upon all the phenomena, interpreted through their relations in the chain of cause and effect from diastrophism to life [pp. 259–60].

THE FIELD OF COSMOGONY

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AS THE nineteenth century opened, it was currently assumed by the learned men of the white race that the heavens and the earth, together with all things animate and inanimate, were created substantially as they appear at the present time in the space of six days by a fiat of Divine Will, approximately six thousand years ago. This cosmogony, which had been current in Europe for many centuries and which had been derived from the leaders of thought of the ancient Hebrews, is but one of a variety of cosmogonies which rest upon the concept of a creation by a Divine Being that has existed through all eternity and that itself required no explanation. It is not necessary here to enter into the fanciful details of such cosmogonies, or to attempt interpretations and explanations; but, on the other hand, it is not possible to ignore the existence of a type of human thought that has dominated the entire past, and still dominates a vast majority of men who regard themselves, at least, as more or less learned. Explorers in the difficult fields of cosmogony from a scientific point of view are still hardy pioneers—as little known to their contemporaries in general as were Vasco da Gama or Jacques Marquette.

When the telescope had revealed the fact that the planets were brothers and sisters of the earth, some larger and some smaller, and the new science of dynamics had made intelligible their apparent motions upon the skies, it was no longer possible to regard the earth as the central body about which the whole universe turned, and it was more difficult to believe that man was the central figure of existence, and that all things had been created for his benefit and profit. Astronomy offered to philosophers a larger, clearer vision than had previously been possible. It presented a very grand, but simple, picture of the solar system—a very large, massive, hot central body, the sun, about which the relatively small planets revolved in ellipses. The motion in these ellipses is definite and regular, in perfect accord

with the three laws of motion and the law of gravitation. The sun rotates about its axis, and the planets do likewise. Most of the planets are accompanied by one or more satellites, and the satellites move about their primaries in accordance with exactly the same laws as the primaries about the Sun.

In addition to those uniformities which demonstrate the dynamical laws, there are other uniformities in the solar system which are very striking and which can be regarded only as links that connect the present state of the system with the past. They are of such a fundamental nature that they seem to relate, not to the immediate past, but to the very origin of the system. There are eight planets which vary in distance from the sun, from three-eighths of an astronomical unit for Mercury, to 30 astronomical units for Neptune. A priori, their orbits might have had any degree of eccentricity from 0 to nearly 1, and their inclinations to the plane of the ecliptic might have been anything from 0 to 180°. Had the sun picked up these planets, so to speak, one at a time, a random distribution of the eccentricities and inclinations would have resulted. Actually, however, the eccentricities and inclinations cluster about the zero point; that is, the orbits are all nearly circular, they all lie very near the plane of the earth's orbit, and they all move in the same direction. Obviously this is not a random arrangement, but a highly systematic one. But, as if to dispel any doubts which might arise upon the matter from so small a number as eight, there are more than a thousand planetoids, small bodies whose diameters range from a few miles to a few hundred miles, that conform to the same system, although not quite so closely as do the planets.

In a general way the rotations of the sun and the planets also conform with the motions of planetary revolution, but there are notable departures. The equator of the sun is inclined 7° to the plane of the ecliptic; for Earth and Mars, 23°–24°; Jupiter, 3°; Saturn, 28°; Uranus, 98°; Neptune, 170°, assuming that the satellites of Uranus and Neptune lie in the equatorial planes and indicate the directions of rotation, which cannot be determined from observations on the planet itself.

The satellite systems of the planets repeat the uniformities in the motions of the planetary systems, although the outermost satellites of Jupiter and Saturn have a retrograde motion.

In his *Système du Monde* of 1796, Laplace suggested that these manifest uniformities could be accounted for by supposing that initially the sun was a vastly distended, very hot, gaseous nebula in slow rotation. As the heat was radiated away, the nebula contracted, owing to the mutual gravitation of its parts; and, since the moment of momentum was constant, the angular rotation was increased. In the course of time the size of the nebula was notably diminished, and its rate of spin was notably increased. Eventually the nebula assumed a lens shape, rotating like a solid body. When the centrifugal force at the edge of the lens became as strong as the gravitational attraction of the entire mass, a ring all around the outer border failed to participate further in the process of contraction. It was therefore abandoned by the contracting mass, and the ring began a separate existence, just as the rings of Saturn have an individual existence.

Gradually the material in the ring gathered together in the more stable form of a sphere—and a planet was born. Eight separate times this process was repeated, and from these rings came the eight planets. In one case the ring failed to concentrate into a single large planet but formed a large number of smaller bodies, the asteroids or planetoids. The larger planets in their process of contraction passed through the same experience as did the sun, thus giving rise to the satellites. Comets and meteors do not exhibit the same regularities of motion as do the planets and satellites. They exhibit, rather, the random distribution of motion that is to be expected in objects which have been picked up one at a time.

Such was the famous "nebular hypothesis" of Laplace. The great fame of its author as a mathematician and astronomer, together with its own inherent simplicity and attractiveness, made its appeal universal during the entire nineteenth century. Professor Chamberlin characterized this hypothesis as "perhaps the most beautiful and fascinating ever offered to the scientific public." It played a rôle the importance of which can scarcely be overestimated in the young sciences which were coming into existence or growing in strength during that period. When the physicists, for example, had clarified their ideas with respect to the properties of energy during the fifth decade of the century, they opened the way for Helmholtz, in 1854, to show that vast quantities of heat would be

generated by a contracting sun, and that, in contracting to its present size, our own sun would have generated sufficient heat to last, at the present rate of radiation, for approximately twenty million years. In 1870 Lane showed, in addition, that as long as the sun was in the state of a monatomic gas, its temperature was inversely proportional to its radius, so that it was no longer necessary to assume, as Laplace had done, that the original nebula was very hot. Even though it had been intensely cold, it would have generated and retained the heat necessary to bring it to its present state in the process of contraction. It is scarcely necessary to remark that the contributions of Helmholtz and Lane were of the utmost importance to the nebular hypothesis. They showed that it was in accord with modern ideas in physics and, furthermore, that it gave a quantitative basis for the time element, a basis which is always desirable and eagerly sought for every hypothesis.

Such, in a brief way, is the contribution of the mathematicians to the hypotheses of cosmogony, a subject that was old and hoary before the science of mechanics had been dreamed of. Their contributions began and ended during the nineteenth century. Whatever one may think of their particular group of hypotheses, one thing at least has been made clear to anyone who would enter this field: No hypothesis of cosmogony, from whatever source it may come, will be deemed worthy of attention unless it satisfies the somewhat exacting demands of celestial mechanics. The choice of hypotheses is thereby strictly limited; but mechanics alone can never quite close the doors to new possibilities, even though it does seem to have the power of excluding one that has already been admitted.

Modern geology began with Hutton, Lamarck, and Cuvier. According to Hutton, it was not the business of a geologist to speculate upon the origin of the earth, or upon its end; his attention should be confined strictly to the geological evidence, in which he maintained there can be found "no traces of a beginning, no prospect of an end." But this advice, wholesome as it is, is too restrictive of the human spirit, for it would deny the possibility, at least to a geologist, of a wider point of view than that given by geology, a point of view in which evidences of astronomy, geology, physics, chemistry and biology unite, under the guidance of an exacting mechanics, in

telling a larger, perhaps much more thrilling, story in which the entire earth plays but a minor part. At any rate, the earth is but a minute part of the solar system, and the solar system is but a single unit in the vast galaxy of stars. It is as impossible, therefore, for the geologist to ignore the general setting in which his science finds itself as for the biologist to ignore the environment of the living organism. Interpretation of the geological evidence necessarily rests upon the point of view which is entertained with respect to this wider relationship.

It is certain, also, that the geologists did fall under the magic spell of the nebular hypothesis of Laplace. This famous hypothesis not only made a strong appeal to the aesthetic sense on account of its simplicity, but, at least during the first half of the nineteenth century, it seemed to meet all of the requirements of mechanics, astronomy, and geology. When, about the middle of the century, the physicists, under the leadership of Carnot, Joule, and Lord Kelvin, developed new ideas with respect to energy, it was found by Helmholtz and Lane that these ideas fitted the nebular hypothesis perfectly, and merely added new beauties to those which had already been recognized. It is small wonder, therefore, that to the scientists of the first half of the century it was a noble hypothesis and that to the scientists of the second half it rose to the grandeur of a tradition and a dogma.

To the geologists this hypothesis indicated that, in its earliest planetary stage, the earth was a liquid globe of molten material surrounded by a dense atmosphere which contained not only all of our present atmosphere but also all of the waters of the earth and the carbon and carbon dioxide which was later locked up in the coal deposits and in the carbonate rocks. In the course of time the liquid globe cooled, and a solid crust, thought to be at present some 30 or 40 miles thick, was formed upon the liquid surface, just as ice is formed upon the surface of water. Because of the loss of internal heat, the globe contracted. In adjusting itself to the shrinking interior, the crust was broken and crumpled. Throughout the geological ages this process has continued and is still in action. To this agency was attributed the mountain- and continent-building process. Certain portions of the crust are heavier than other portions, and the

heavier portions were thought to have sunk deeper into the liquid below, forming the depressions which hold the ocean waters, the lighter ones rising to form the continents.

Other changes were going on in the atmosphere. When the temperature was sufficiently reduced rains began to fall, and the long process of erosion began. The original plutonic surface was worn away and the detritus, deposited in lakes and oceans, formed the stratified rocks. The early climates were warm and moist, and there was much evaporation and precipitation. Life eventually appeared upon the hitherto barren and desolate surface. Microscopic at first, the living forms slowly increased in size and complexity, and differentiated into the vegetable and animal types. The warm, moist climate of the Carboniferous period, with its atmosphere laden with carbon dioxide, was extremely favorable to the vegetable life, and the forests and swamps were luxuriant. Buried beneath deposits of muds and sediments, the swamp growths were converted into coal. The carbon which they contain was taken from the atmosphere, so that the climates of succeeding geological periods were colder and drier; and if this depletion of carbon dioxide finally resulted in the great ice sheets of the Pleistocene, it was thought not surprising. The earth was believed to be destined to a cold and frigid end. As Professor Chamberlin himself expressed it, the Pleistocene "was but an October frost; December was yet to come."

It was in the field of glaciation that Chamberlin won his first reputation as a geologist. This particular direction of his activities was guided largely by the accidental fact that Wisconsin, where he had been raised and educated and in which he had spent nearly his entire life, had been heavily covered at several different times by thick fields of ice, and the evidences of glaciation were abundant nearly everywhere. It was not sufficient to him to work out and describe the comings and goings of these strange ice invasions. There was the ever-insistent question of why these invasions had occurred, and the puzzle was tremendously increased when it was recognized that similar, even greater, periods of glaciation had occurred far back in Permian times and geographically nearer to the earth's equator, and that there was good evidence of glaciation even at the base of the Cambrian.

The theory that the great ice fields of the Pleistocene had re-

sulted from a gradual depletion of the carbon dioxide of the atmosphere in the formation of the coal and limestone rocks, accorded in a general way with the hypothesis of Laplace. But why had the process of refrigeration been reversed, as evidenced by the disappearance of the ice sheets? What has caused the return, again and again, of more genial climates? Why were not the successive returns of the ice invasions characterized by a more and more intense refrigeration rather than by a diminishing intensity? The general character of the successive ice invasions is not in harmony with the theory, and Chamberlin's faith in the idea that the earth was rather rapidly approaching a frigid end was weakened; and when the evidence for even more remarkable episodes of glaciation in the ancient geological periods was beyond doubt, faith in the Laplacian hypothesis as a basis for a theory of climates was difficult indeed. In 1897 he said: "The nebular hypothesis correlates a wonderful array of remarkable facts and has gained a profound hold upon the convictions of the scientific world, yet some of its great pillars of support have recently weakened or have fallen away entirely." Not only had glaciation occurred far back in geologic time, contrary to the theory that the early climates were warm, but evidence had also been accumulated of aridity at certain times in the early geologic record. Not only is the climate not extraordinarily dry at the present time, but the driest epoch apparently was as far back as the Permian. The theory of climates growing out of the Laplacian hypothesis is clearly out of harmony with the general outline of the facts. The coldest, driest period occurred far back in geologic time.

Professor Chamberlin was dealing with these matters in a series of papers in the *Journal of Geology* as the nineteenth century was drawing to a close. He proposed a modification of the carbon-dioxide theory in which the proportion of carbon dioxide in the atmosphere varied from time to time, and the climate varied with it; but the Laplacian concept had no place in this theory. Indeed, his faith in the entire Laplacian structure had been so weakened that he began to search for a new hypothesis of the origin of the earth. Criticism alone is not sufficient, but

> to admit a competitive hypothesis to the working list is a concrete form of embodying a doubt respecting existing hypotheses, and serves better than any abstract skepticism to keep alive the sources of doubt. I assume that the system

of multiple working hypotheses is accepted as furnishing the most wholesome conditions for research, and that any additional hypothesis not in itself incredible will be welcomed.

In his paper entitled "A Group of Hypotheses Bearing on Climatic Changes," he wrote:

> But if we question current conceptions we should present alternatives which account for the atmosphere. Let us therefore hastily follow the hypothetical growth of a planet built up by the slow aggregation of small bodies which join it at small velocities and develop a minimum heat. Let the case be purposely made rather extreme to develop sharply the difficulties springing from it. Let the infalling particles be small and their rate such as not to generate a high surface temperature. The growth of such a body up to the size of the moon may be taken as an hypothesis of lunar history, and the phenomena of the moon may serve as a check upon it. The moon may, however, have originated by fission even though the earth were built up by accretions. In the early stages of growth the gravity being low the aggregation may be supposed to have remained uncondensed. Volcanic aggregations of bombs, cinders and ashes are perhaps the nearest terrestrial analogues. The ingathering particles obviously carried with them so much of the atmospheric material as was entrapped or occluded within them in their solidification, or was absorbed into their pores or adhered to their surfaces. Judging from meteorites the amount of this might have been large. Gaseous molecules moving as independent bodies may have joined the aggregation and become absorbed in its porous body, but they would not have been collected into an appreciable atmospheric envelope until the body passed the size of the moon if the molecular considerations urged earlier in this paper hold good, though an atmospheric envelope would not have been entirely absent. As the mass grew the central pressure increased and condensation produced heat at the center in proportion to the work done, I find the explanation of internal heat chiefly in this self-condensation, it being essentially the application of the Helmholtz solar theory to a solid body. Tidal kneading and chemical action doubtless added their contributions. . . . .

This passage contains the germ of the idea which, in its mature development, Professor Chamberlin called the "planetesimal hypothesis": the idea that the earth has grown to what it now is by a process of gradual accretion of meteoritic or planetesimal material from a much smaller mass, or nucleus, and that in this process the earth has remained solid throughout, and its surface relatively cool.

The development of the new hypothesis required a re-examination of the entire field from the standpoint of both astronomy and geology. On the astronomical side it was necessary to examine critically the dynamical foundations and consequences of the hy-

pothesis of Laplace and to find out whether the weaknesses which had developed in its geological aspects extended also to its astronomical aspects. Then, too, if the earth was built up relatively slowly, by a somewhat steady ingathering of planetesimal material, it was necessary to find somewhere in the astronomical field an adequate supply of such material.

Professor Chamberlin had not been trained in the rigorous methods of celestial mechanics, his training having been that of the naturalist. It happened, however, that in the department of astronomy of the University of Chicago there was a young instructor, Dr. F. R. Moulton, who was already interested in the astronomical implications of the doctrine of Laplace and who had begun to question its validity on purely astronomical grounds. The ideas of each were made known to the other by students who were attending courses in both astronomy and geology. Conferences were arranged, and there began not only a collaboration that was extremely fruitful but an intimate friendship that was terminated only by death.

The first fruits of this co-operation were two papers in the year 1900: "An Attempt To Test the Nebular Hypothesis by the Relations of Masses and Momenta," by T. C. Chamberlin, in the *Journal of Geology;* and "An Attempt To Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics," by F. R. Moulton, in the *Astrophysical Journal.* Each of the two authors found that the Laplacian hypothesis was not able to bear even the astronomical evidence that was put upon it. The two papers were not independent, however. In his paper, Professor Chamberlin said:

> These results are the outcome of a joint inquiry by Dr. F. R. Moulton and myself. They are a part of the results of a more or less continuous study on related themes lying on the border-land of geology and astronomy, running through the past three years. Our relations have been so intimate and our exchanges of ideas so free and so frequent that it is impossible to apportion the responsibility for the various methods adopted and the modes of carrying them out. The higher mathematical work is, however, to be credited to Dr. Moulton. It has perhaps been my function in the main to formulate problems and suggest general modes of attack, and Dr. Moulton's to devise methods of analysis and bring to bear the mathematical principles of dynamics, but this has not been uniformly so. Quite often we have proceeded by successive alternate steps in which each was the parent of its successor.

There is also a similar statement in the paper of Dr. Moulton's.

The next step was a study of possible sources of meteors, planetesimals, and dispersed matter in general. The results of this study were contained in a paper by Professor Chamberlin which was published in the *Astrophysical Journal* in 1901 under the title "On a Possible Function of Disruptive Approach in the Formation of Meteorites, Comets and Nebulae." Its central theme was the effects of the close approach of two stars without collision, and in it he showed how such an approach would result in the dispersion of a portion of the sun's own material, and a possible fragmentation of already existing solid bodies such as planets.

On the geophysical side, there were problems relating to the origin and distribution of the internal heat of the earth; the sources of the hydrosphere and atmosphere; atmospheric balance according to the kinetic theory of gases; changes in the rate of rotation of the earth; internal pressures, compressions, and molecular rearrangements; the elasticity and rigidity of the earth; effects of the solar and lunar tides; and so on. No one person could be competent to deal authoritatively with all of the many problems which were raised by the new point of view, nor would he have the time to deal with them even if he were competent. For this reason Chamberlin sought the advice and assistance of experts in many of the fields of physical science, such as Dr. Lunn, Dr. Stieglitz, Professor Hoskins, Professor Schlichter, and many others whose names are not recorded here. He was very eager for help and for suggestions, and very generous with acknowledgment of any assistance which he had received. A collection of papers relating to these problems by various authors was published in 1909 as *Publication No. 107 of the Carnegie Institution of Washington*, under the title "The Tidal and Other Problems."

Although the new hypothesis had been widely discussed with and by scientific men in this country from the year 1900 on, its first appearance in print with a full discussion was in the *Year Book No. 3 of the Carnegie Institution of Washington*, for the year 1904. Other accounts of the hypothesis appeared shortly afterward in Chamberlin and Salisbury's *Geology*, Volume II (1905), and in Moulton's *Introduction to Astronomy* (1906). In the introduction to the discussion in the *Year Book* of 1904, p. 210, Professor Chamberlin says:

As the basis for developing the typical form of the planetesimal hypothesis, I have assumed that the parent nebula had a planetesimal organization from the outset. The conception is a rather radical departure from the gaseous conception of the familiar Nebular Hypothesis, and from the meteoritic conception of Lockyer and Darwin, so far as fundamental dynamics, and mode of evolution are concerned. To develop the hypothesis as definitely and concretely as possible, I have further chosen a special case from among those that might possibly arise, viz., the case in which the nebula is supposed to have arisen from the dispersion of a Sun as a result of close approach to another large body. The case does not involve the origin of a star, nor even the primary origin of the solar system, but rather its rejuvenation and the origin of a new family of planets. The general planetesimal doctrine does not stand or fall with the merits or demerits of this special phase of it, but to be of much real service in stimulating and guiding investigation, a hypothesis must be carried out into working detail so that it may be tested by its concrete and specific application to the phenomena involved, and hence the reason for developing a specific sub-hypothesis. This particular sub-hypothesis was selected for first development (1) because it postulates as simple an event as it seems possible to assign as the source of so great results, (2) because the event seems very likely to have happened, (3) because the form of the nebula supposed to have arisen in this way is the most common form known, the spiral, and (4) because spectroscopic observations seem at present to support the constitution assigned this class of nebula, although it must be noted that spectroscopic observations have not reached such a stage of development as to demonstrate the motions of the nebular constituents.

It is evident from this passage that the fundamental hypothesis was the existence of a nebula in which the motions of the particles were dominantly revolutions about a central mass rather than dominantly of the gaseous type of motion. This was the hypothesis to which his geologic and climatic studies had led him, and to which he clung tenaciously. The organization of such a nebula by the disruptive action of a passing star was, at first, a subhypothesis—a hypothesis to be replaced by an alternative in case it failed. The distinction between the hypothesis and the sub-hypothesis is highly illuminating. It marked clearly the geological character of the path along which he had arrived at his goal. Had he been an astronomer, had his thoughts been riveted upon the stars, it is altogether probable that the rôle of hypothesis and sub-hypothesis would have been interchanged.

But the sub-hypothesis did not fail in its purpose of harmonizing those simple and conspicuous relations among the attendants of the

sun that had engaged the attention of Kant and Laplace. The facts that the orbits of the planets are circular, that they lie almost in a single plane, and that the planets all move in the same direction about the sun, follow as corollaries from the hypothesis of Chamberlin as simply as they superficially appear to do from the hypothesis of Laplace. Indeed, a careful scrutiny of the hypothesis of Laplace fails entirely to reveal what would happen. It is very doubtful whether there would have been any planets at all. According to the hypothesis of Chamberlin, the common plane of the planetary motion is nearly identical with the plane of the passing star, and the direction of the planets' motion is the same as the direction of motion of that star relative to the sun.

According to the hypothesis of Laplace, the present period of rotation of the sun should be but a few hours, and its axis of rotation should be perpendicular to the common plane of planetary motion; and neither of these expectations is in accordance with the facts. The period of the sun's rotation is nearly 26 days, and its axis differs by 7° from its expected position. According to the hypothesis of Chamberlin, the present rotation of the sun is the combination of its unknown ancient rotation and an unknown rotation about an axis perpendicular to the plane of the passing star which was impressed upon it by the passing star. The fact that the sun's axis is within a few degrees of this perpendicular suggests a preponderance of rotation impressed by the passing star, even though its actual position can be explained otherwise. The sun's rotation, however, is not a critical point under this hypothesis.

There is a slight tendency under the assumed mode of origin for forward rotation of the planets, but the preponderance is very small. If the rotations of Earth, Mars, Jupiter, and Saturn are forward and relatively rapid; if the rotations of Venus and Mercury are forward but very slow; and if the rotations of Uranus and Neptune are backward, as they seem to be, about all that can be said is that forward rotation preponderates; but again, the point is not critical, for initial individual peculiarities of rotation might easily outweigh statistical expectations.

Professor Chamberlin did not regard the formation of the satellite systems as a repetition in miniature of the formation of the

planetary system. The nuclei of the satellites were from the beginning, or at least from very near the beginning, companions to the nuclei which later developed into the planets. They shared, therefore, in those events which controlled the rotations of the planets. If our own moon and a few of the outermost satellites of Jupiter and Saturn are excepted, the relations between the revolutions of the satellite systems and the rotations of the primaries seem to be rather closer than would have been anticipated. The satellites mentioned, however, are markedly exceptional.

It is seen from the foregoing brief sketch of the bi-parental hypothesis of the origin of the planetary system, to use another of Professor Chamberlin's phrases, when the origin of the planetesimals is under consideration, that the hypothesis accounts perfectly for those features of the system which stand out boldly and attract the attention, just as in a broad way modern geological theories give a satisfying account of the existence of mountains and valleys. Just as accidental peculiarities have determined the positions and shapes of the individual mountains or valleys, so the accidental distributions of nuclei and planetesimals and their velocities have determined the individual peculiarities of the various members of the planetary system.

The Laplacian hypothesis was highly attractive to mathematicians because its simplicity attracted the efforts of mathematicians, and its difficulties stimulated their best efforts without ever wholly satisfying them. It became almost a dogma that cosmogony was a mathematical subject. Anything would succumb under a sufficiently acute mathematical attack. It is evident, however, to one who meditates much upon the violence which attends the birth of a planetary system according to the bi-parental hypothesis, that the contributions of the mathematician, while extremely valuable, must always be limited to broader features only. Just what will occur when two hot, intensely energetic, rotating stars pass each other at relatively small distances on hyperbolic orbits, is a very beautiful mathematical problem, but it is an extremely complicated one. If the eruptive features of real stars are included, it ceases to be a mathematical problem, for it is no longer definite. The problem is much simplified if the stars are assumed to be quiescent and not

rotating. It is conceivable that such a problem might be solved if the distance of approach were not too small. While the solution would give much valuable information, it would still be only an approximation, and this approximation would doubtless be greatly modified when the elements of eruption and axial rotation are included.

It is evident, therefore, that with the planetesimal hypothesis, or better perhaps with the bi-parental hypothesis, cosmogony ceases to be a purely mathematical subject. The problem, complicated by many elements, requires the insight and the genius of a great naturalist, precisely such talents as Professor Chamberlin possessed; and when the lively imagination of genius is regulated and controlled by another who is highly gifted in the difficult fields of celestial mechanics, there is a concurrence of talents that is rare indeed. Such collaboration existed between Professor Chamberlin and Dr. F. R. Moulton through many years of intimate association. Indeed, in the end, Professor Chamberlin himself acquired an insight into and a mastery of the principles of mechanics that was highly remarkable when it is borne in mind that he was approximately at the age of sixty when these studies were commenced.

In the field of geology he was, of course, a master. That the planetesimal hypothesis met the demands of geology was to be expected, for it was the needs of geology that gave it birth. It is particularly evident that, on the basis of this hypothesis, glacial epochs are no more surprising in the Cambrian or in the Permian than in the Pleistocene. We may be in doubt as to why they have occurred at all, but at any rate the geological evidence does not tell us that there was glaciation at a time when the theory tells us that the climate was warm and moist. Again, the theory does not tell us that in the remote past the earth's rate of spin was much higher than it is at present, and therefore the day much shorter than now, when the geological and geographical evidence is all against it.

The geological and the biological evidence is all against the few million years that were allowed for the age of the earth, even by so eminent a physicist as Lord Kelvin, who regarded the age of the earth as rigorously limited by the theory of Helmholtz. To be sure, the origin of the sun's heat has nothing to do with the planetesimal

hypothesis, further than the fact that Professor Chamberlin refused to acknowledge the validity of Helmholtz's theory when it ran counter to the geological evidence. He postulated a condition of the sun quite similar to the present one at the time when the planetary system was formed, perhaps billions of years ago. In doing so, he broke the fetters of astronomical and physical dogma just as truly as Galileo broke the fetters of theological dogma. He did not worry over the question of the origin of the sun's heat. It was evident to him on geological grounds that the sun had been in substantially its present condition throughout the entire history of the earth, perhaps billions of years. It was a problem for the physicist to discover the source, or the sources, of its energy. Nevertheless, thirty years ago, when Lord Kelvin asserted, in his Baltimore address, that gravitational potential was the only source of the stellar energies, Professor Chamberlin pointed out possibilities of subatomic energies which, he suggested, might be of a very high order of magnitude. The physicists at that time had not recognized these energies; but when the geological evidence seemed to demand them, Professor Chamberlin did not hesitate to suggest them and to deny the right of a physicist to assert, even implicitly, that only those things exist with which he, the physicist, is acquainted.

During the past thirty years, however, the physicists have succeeded in penetrating many secrets of the atoms and have given us the very wonderful electron theory of its structure. According to this theory, mass is a property of the electron and is due to the electric charge. The available electrostatic potential energies of its electrons are perhaps hundreds of thousands of times greater than the available gravitational potential energy of the sun, and dynamical astronomers no longer heed the time limitations which had been imposed by the theory of Helmholtz. Indeed, the dynamics of star clusters and galaxies are vastly more extravagant of time than is the science of geology.

It is greatly to the credit of Professor Chamberlin that in a broad way he anticipated these results. He stated that the close approach of our sun to another star was a likely event, because his mind had been freed from the fetters of the Helmholtz theory. To the astronomers who had been reared on the theory of Helmholtz, an

event which occurs on an average only once in a million billion years seemed highly unlikely; and this divergence of view as to the time-scale was probably the greatest obstacle which the planetesimal hypothesis had to encounter. According to the nebular hypothesis of Laplace, all of the bodies in the solar system evolved from a common nebula which had once existed in a vastly diffused state. In a hazy way it was thought that all the other stars had done likewise, so that if there had not been a creation a few million years ago, at least all matter everywhere was in a state of diffused nebulosity, and it was only within the past few million years that galactic, stellar, and planetary organization has occurred, a simultaneous organization everywhere.

This was not the view of Professor Chamberlin. As he saw it, the galactic organization of stars was already very old when our present system of planets was formed, and the process of forming planetary systems has gone on with other stars in our own galaxy and in other galaxies through all past time and will continue throughout all time to come. He has led us up to the summit of a very high mountain, from which we have a clearer perspective of great and distant events, and a vision that will always bring joy to those who can live and breathe in the rare atmosphere of such lofty elevation.

"DYNAMICS IS THE SOUL OF THE PROBLEM"

BAILEY WILLIS
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IN 1897 Chamberlin published *A Group of Hypotheses Bearing on Climatic Changes,* the first to appear of those studies which for several years before that date had led him to question the generally accepted doctrines then current among geologists: the Laplacian hypothesis of planetary genesis and its terrestrial implications. The latter included the idea of a molten earth and a formerly dense atmosphere, rich in carbonic acid.

Carbon dioxide is now a very small but critical component of the atmosphere, which has heat-retaining properties of such potency that moderate variations of the proportion of it to the other gases would suffice to produce material changes in climatic temperatures. Tyndall had thrown out the suggestion and Chamberlin was testing it as a possible cause of glaciation. Tried according to standard interpretation, it was too potent. The atmosphere in its early stages, before the deposition of carbonates and carbonaceous accumulations, must have contained from 20,000 to 30,000 times the present content of carbonic acid and have been exceedingly dense, moist, and warm. On the other hand, the residue remaining after the carbon had been locked up would not suffice to meet the requirements of depleting agencies more than 10,000 years, more or less. This enormous change in the proportion of the critical component required a corresponding change in temperature, from excessive heat to extreme chill. The facts of the sedimentary and biologic record indicated nothing of the kind; rather they suggested fluctuations about a mean not materially different from the life-giving conditions of today. The evidence of early glaciations subsequently sustained these doubts and they have been strongly confirmed.

The inconsistency of the logical deductions regarding the atmosphere with the facts of geologic history led to further doubts of the Laplacian hypothesis itself, because they were inherent in the dynamics of the case. A study of molecular velocities, in the course of

which Moulton and Whitney confirmed the earlier estimates by Johnstone Stoney, showed additional divergencies between the unavoidable results of the hypothesis and the fact of the presence of hydrogen in the air and water of the globe, since that gas must have been in large part lost by centrifugal discharge from the highly heated, rapidly revolving sphere of gas. The facts required low temperature and a relatively limited speed of revolution; they pointed directly to the evolution of a solid globe rather than of a molten one.

In 1897 the planetesimal hypothesis had not been conceived, but we thus find intimations of it in the article on climatic changes. Chamberlin reasoned that the earth-moon ring postulated by the Laplacian hypothesis would probably have become cooled to solid, discrete particles while still in the ring form; that the aggregation into a sphere would be accomplished slowly by overtake collisions between particles traveling in the same direction, and that the heat generated by any one impact would be lost before another could take place. He was already picturing the growth of the solid earth.

One of the implications of growth by the infall of solid particles is that atmospheric gases should be trapped and a more or less abundant volume of them should be available to feed the external atmosphere when the planet should have grown to a mass sufficient to hold one. The balance of proportionate supply and demand could not be struck, but the reality of the internal sources was evidently an important fact, and it remains so.

Chamberlin described an early stage of the growing planet when it might be compared to the moon, as embracing:

> (1) a dense central portion raised to high temperature by compression, giving a potentiality of liquefaction by relief of pressure; (2) a zone of declining temperature and less compressed structure, graduating toward a porous condition; and (3) at the surface the still unconsolidated open aggregation.

From this concept, which departed entirely from that of the cooling, shrinking globe, he deduced conditions favorable to explosive vulcanism and found confirmation of the suggestion in the craters of the moon, which he had studied independently. But even though he seemed to be on the right track, the disciple of the method of multiple hypotheses merely added to his list of working postulates.

Chamberlin was well across "the pass that leads from the land of rocks into the realm of cosmogonic mists" and the light was breaking through the mists ahead.

The dynamics of the atmosphere, analyzed in the light of the kinetic theory of gases, had thus contributed very materially to the discredit of accepted views and the suggestion of alternatives. Two paths of investigation opened. The one continued toward the discovery of the causes of climatic changes, the other toward the formulation of a consistent theory of the genesis of the earth and other planets. Chamberlin followed the former as far as the application of world-wide postulates could carry general reasoning. He evaluated the significance of variations in the composition of the atmosphere, especially with reference to the critical constituents, carbon dioxide and water vapor; he traced the processes of change in the chemical, biological, and orogenic cycles, and he found an initial cause in periodic diastrophism, due to the earth's internal forces. This solution of the problem of the cause of glaciation is not final. It does but take its place, as he would wish it to, among the working hypotheses that may reasonably be considered. More exact knowledge of diastrophism, especially as to its distribution over the globe and its periodicity, is needed. More complete maps of paleogeographic conditions that affect climate are essential. Better understanding of the conditions that affect atmospheric circulation must follow. And the courses of ocean currents under each set of controls must be worked out.

To this last named subject, the influence of oceanic circulation, Chamberlin made a most suggestive contribution by suggesting a possible reversal of the vertical movements of ocean currents. At the present time tropical warmth and polar cold control. The cold waters sink and occupy all deeps, the warmed waters float and flow poleward, losing heat as they go. The system is a cooling one. Were the warm waters sufficiently saline to exceed the cold waters in density, they would sink in equatorial regions and carry their heat toward the poles, thus establishing a warming system. The existing undercurrent from the Mediterranean into the Atlantic illustrates how close is the balance.

In the major problem of earth genesis Chamberlin and Moulton

were guided by the study of the dynamics of the solar system to the recognition of that prime cause which is now generally accepted, a dynamic encounter of the sun and a passing star. This fundamental discovery was again the result of painstaking analysis of the realities. It was in no sense a speculative concept. It was not a flash of genius. Rather was it the child of that genius which is characterized by intensive attention to details and facts.

In testing the hypothesis of Laplace the indefatigable investigators had not been content to have proved it inconsistent with the kinetic theory of gases. They sought further proof or disproof in the laws of mechanics as applied to rotating, shrinking bodies. Briefly, the velocities of the planets in their orbits about the sun indicate the speed of rotation that a Laplacian nebula must have had when of the radius of the corresponding orbit and thus make possible a calculation of the speed of rotation the sun should now have. It appeared that the equatorial velocity of the sun should now be 270 miles per second instead of the actual $1\frac{1}{2}$ miles. It follows cogently that the sun cannot be the residual sphere from a Laplacian nebula, or any similar rotating gaseous body from which the planets might be supposed to have been thrown off.

The reasoning may be reversed. Given a slowly rotating sun, it could not by virtue of its rotation have imparted to the planets their high velocities of revolution in their orbits. How could the latter have been acquired? This question was endowed with peculiar significance when the relative masses of the sun and planets were contrasted with their relative momenta of momentum. The sun possesses 744/745 of the whole mass. The planets have 49/50 or 730/745 of the total momentum. This extraordinary disparity pointed to the action of another body, which must have acted upon the sun in such manner that while the sun yielded up a small fraction of its mass, the several parts of that fraction were endowed by the other body with the velocities of revolution possessed by the several planets.

Roche had previously demonstrated the possibilities of disruptive attraction in the event of two stars approaching each other, so the problem that presented itself to Moulton was to define the relations of mass and distance that might satisfy the actual conditions

of the planetary system on the assumption that a passing visitor had caused the separation of the planets from the sun. There being two variables, the mass of the star and the least distance between it and the sun, their possible combinations covered a wide range and the competency of the effect was demonstrable. This is true even assuming that the tidal attraction of the star be taken as the only effective force. The assumption presents one line of possible planetary evolution, though an improbable one because of the implied postulate that the sun itself remained inert during the process of disruption. The idea, which has attractions for the theorist because it can be reduced to mathematical terms, has been elaborated by Jeans and Jeffreys,[1] who came forward fifteen years after Chamberlin and Moulton had deliberately chosen the alternative path of research, that involves the active co-operation of the sun. To disregard the latter was to embrace an unreal concept and vitiate the conclusion.

The sun's co-operation in the birth of the planets must depend upon certain known forces and upon others that were surmised to exist and are now in part known: radiation pressure, electrical repellencies, and atomic activities. In its present state the sun is powerfully eruptive by reason of the energy set free, in part at least, by its own self-compression. This energy has increased in intensity as the sun has condensed, provided the sun be assumed to have been less dense at some earlier stage, but at any stage the eruptive activity could not have failed to have been greatly stimulated by the stresses of compression and tension set up by the passing star. To disregard that activity is, therefore, to disregard a reality.

Chamberlin's sense of reality was with him a dominating instinct. He had already, in 1899, and in fact even as early as 1895, anticipated the discovery of atomic forces of greater potency than the energy of gravitative compression. He could not deny them. He has been abundantly justified by the discoveries in the atomic and electronic fields, and it is more than probable that his confidence in their capacity to produce comets from the sun, as expressed in his last great work, *The Two Solar Families*, will eventually be

[1] J. H. Jeans, *Problems of Cosmogony and Stellar Dynamics* (Cambridge Press, 1919); Harold Jeffreys, *The Earth* (Cambridge University Press, 1924), pp. 16–35.

proved to have been guided by that profound insight which so strikingly characterized his research.

Probably at no time in his career as an investigator did Chamberlin more courageously apply the method of multiple hypotheses than when, as a veteran of eighty odd years, he undertook to review and revise his work on the origin of the earth. The philosophy represented by the planetesimal group of hypotheses had been before the scientific world for more than a quarter of a century. Objections to it had been raised and alternative derivatives had been proposed to replace the particular line of development that Chamberlin and Moulton had regarded as the more consistent with facts. But in every case, the objection or the alternative theory had, upon examination, been found to rest upon less comprehensive analysis or upon misinterpretation of the dynamics. The continued, critical researches of the authors themselves, keeping step with advances in physics and astronomy, had failed to invalidate the reasoning upon which the planetesimal theory rests. Under these conditions the senior investigator might well have been content to assume that the theory met the facts of nature and was really true. But he made no such assumption. He proceeded to rebuild it, testing each apparent fact, each inference from accepted fact, in the light of the latest understanding.

In the course of this reconstruction of the planetesimal hypothesis, Chamberlin came upon a "lion in the path," a lion of very menacing aspect. It became evident that a planetary bolt, if shot from the sun by the eruptive activity of that body in response to tidal stimulus, must be endowed with cyclonic rotation around the axis of the bolt. Complex though the spiral motions within the bolt would be, a dominant motion of rotation around the axis seemed inevitable. But the plane of that rotation would lie at right angles to the orbital plane of revolution around the sun, whereas the actual planes of rotation of the planets more nearly approach parallelism with the orbital. Here was an apparent contradiction of grave import, but, as Chamberlin afterward said, there was nothing to do but to face it, follow it, and see where it led. "The truth cannot mislead." Those who wish to follow the lion hunt are referred to the chapter in *The Two Solar Families* in which the growth-creep of the axis is

discussed. Accurate analysis of the conditions governing the infall of planetesimals upon the core showed that the growth must be unsymmetrical with reference to the initial axis, and that the latter would creep accordingly to maintain rotation about an axis of symmetry. Calculations demonstrated that the creep from the probable original position to the actual one was within reasonable limits for the causes assigned. The probable original position of the axis is deduced from the arrangement of the land and water hemispheres on the ground that that arrangement represents a difference in density inherited from the lighter and heavier ends of the bolt from the sun.

In revising the planetesimal hypothesis, Chamberlin became more than ever impressed with the potency of the eruptive activity of the sun. He was confirmed in his previous estimates by the recent observations which indicate that the repellent forces nearly equal the attraction of gravity at the sun's surface. While it is true that the nature and origin of the repellencies are still obscure, their capacity to project masses of cometary magnitude to cometary distances appears indicated as probable. Chamberlin was therefore led to consider the comets as children of the sun. With searching insight and mastery of the natural logic of the dynamics, he co-ordinated the phenomena related to these eccentric projectiles, and contrasting their individual aberrancies with the orderly family of the planets, he embraced within one consistent hypothesis the explanation of the origin of both solar families. This, his final advance in research, constitutes a masterful achievement in the domain of celestial mechanics as applied to the solar system.

Very early in that searching investigation from which the planetesimal hypothesis developed, it became evident that a major difficulty lay in the aggregation of the planetesimals to form planets. Chamberlin had already encountered this problem in 1897 when considering how a Laplacian ring might assemble. He then saw no solution. In the course of subsequent studies, in co-operation with Moulton, he became convinced that the dynamic tendencies of gaseous masses presented grave obstacles to self-assemblage. This conviction grew upon him as evidence accumulated of the eruptive tendencies of such bodies and the weakness of their conservative

powers, except in very large masses. Orbital organization he recognized by contrast as the type of conservatism, having in favor of ultimate assemblage the advantage of immense longevity, which gives opportunity for the action of minor or incidental tendencies toward the combination of particles or parts. Therefore he regarded the dynamics of orbital organization as most favorable to the growth of a planet from the planetesimal swarm to the completed spheroid.

The dynamics of a swarm of planetesimals pursuing each other in subparallel orbits around the sun at different speeds do not attract the mathematician. The factors that determine independence or collision are too numerous and too indeterminate. Picture a vast number of automobiles moving around a central point on a broad but crowded one-way highway. They would collide by side-swipe and overtake one another at points where their paths intersect. The chances of collision might be formulated if they had been started from a common line in parallel trajectories, with given tendencies to approach each other. But the mathematical analysis fails when tried in the case of a swarm that inherits the turbulence of cyclonic expulsion from the sun. This is one of those cases in which we have to accept the preponderance of chance in favor of the actual result, even though among philosophers of exact reasoning the conclusion remains open to question because they cannot reduce it to the narrow measure of their Procrustean bed. Nevertheless, Chamberlin did not hesitate to accept the challenge and to show that, under certain assumptions of artificial orderliness, the swarm would ultimately assemble by overtake collisions at mathematically determinable points. Even so, he went further. Applying his constructive imagination to the problem, he elucidated by "naturalistic logic," with extraordinary clearness, the complexities of solar eruptions and planetesimal, orbital dynamics. He investigated the conditions antagonistic to the formation of a core, isolated the more unusual conditions favoring it, and clearly traced the process of assemblage of heavy molecules, which was followed by the slow growth of the planet as it gathered in the lighter ones.

Chamberlin's revision of his published views did not extend to those relating to the diastrophism of the globe. At the time of his death he contemplated a searching analysis of the major geophysical

problems and he had laid down some of the lines of investigation in the series of articles on "megatectonics." He held as working concepts the ideas published in *The Origin of the Earth* in 1916, but it was obvious in personal discussion that he held them as working concepts only and was hospitable to alternative suggestions. He welcomed the new lines of constructive hypothesis that are opening out as advances in tectonics, seismology, and petrology. Just as he had examined critically the hypotheses of Laplace, Lockyer, and George Darwin on the origin of the earth, so he was investigating the ideas of Hayford and Bowie on isostasy, of Barrell on the asthenosphere, and of others on the thermodynamics of the earth. He appeared to pay less immediate attention to the reasoning of petrologists, though not to the facts of petrology, and the distinction may perhaps be explained by the fact that they adhere to the idea of a molten globe, which he had definitely discarded.

We may not doubt that had he carried on his patient, productive research, he would have continued to distinguish "crustal tectonics," as he called the superficial theories of diastrophism, from "megatectonics," which deal with deep-seated terrestrial deformation. The former embody the notions of a collapsing crust or of delicately adjusted, balancing masses in a globe devoid of rigidity. He had tested such ideas and found them inconsistent with the evidence of a very solid, rigid, and elastic earth. The concept of the growth of the planet, which he held firmly, directed his attention to the interior of the globe in the search for the beginnings of geologic processes. Recognition of the slow march of those processes extended his view telescopically down the immense vistas of time and gave definition to remote, long-buried conditions. The immensity of duration of terrestrial eons suggested the potency of slight, but persistent activities. We have seen this principle of the effectiveness of the infinitesimal in the infinitely prolonged at work in the process of planetesimal assemblage. Flakes of snow make glaciers. The dust particles gathered to form the globe. The concept runs through the web and woof of Chamberlin's thought like a golden thread of truth. Let us follow it quickly.

The growing earth must have increased in mass from the stage when it could not hold an atmosphere to the stage when it could.

Whenever the atmosphere acquired the active gases, oxygen, carbonic acid, and water vapor, the atmospheric processes began. Rock weathering being intiated, the siliceous concentrates of weathering must have been produced and have gathered in subaerial or subaqueous deposits. When sufficiently buried and heated in the growing earth, the concentrates must have formed relatively acid magmas, masses of relatively low density. If the conditions of weathering continued to persist over a given geographical area, or if they did so during a succession of ages, there grew up under that area a comparatively light segment, which contrasted with the denser segments adjoining it, whose surfaces were protected by water from the weathering action.

Chamberlin's studies of atmospheric agencies as possible causes of glaciation had given him an intimate knowledge of the atmospheric circulation. He knew the tendency of great air currents to follow spiral courses around the hemispheres and to develop permanent centers of high and low pressures. He visualized these agencies at work during the slow growth of the planet by infall of planetesimal dust, and he credited them with a sifting, screening power they undoubtedly possess. Lighter and heavier particles of dust separate in dry winds, but dust is carried down unsifted by rains. Here is an elusive, subtle, but real effect, which may have co-operated in building the continental segments. The suggestion illustrates at once the delicacy of the thinker's analysis and also the boldness with which he regarded the possible influence of slight causes when summed up algebraically during eons of time.

The investigations of tidal stresses carried out by Sir George Darwin definitely influenced Chamberlin's views upon diastrophism of the globe. He repeatedly cited the conclusion that the stresses are greater in the interior than nearer the surface and he deduced an initial segmentation from their action. The appropriate orientation of shearing coincides with important sections of the outlines of continenal masses and strongly suggests a relation of cause and effect. The stresses must have affected the growing globe from an early stage on, and the continental outlines, once established, might be expected to persist and to be represented in the actual continents.

Here are at least three causes of differentiation of lighter and denser segments of the earth's mass, weathering, atmospheric sift-

ing, and tidal strains, which have persisted during the growth of the envelope around the core. The distinction between the segments thus built up is one of density. Both heavier and lighter have been compressed, but inasmuch as the load on the latter was less, the degree of compression has been less. The lateral stresses have also been less from the lighter against the heavier than vice versa. Thus the surfaces of contact of lighter and denser segments are surfaces of opposition of smaller and greater stresses. Chamberlin regarded them as surfaces of weakness and also of concentrated stresses and attributed those larger phases of diastrophism which we may include under "megadiastrophism" to their development. In weighing the significance of these concepts, whether as indices of his line of research, or as suggestions for further investigation, we must bear in mind that they were but working hypotheses, which were to be tested by every advance of knowledge and were acceptable to him only so long as they could make good.

Elsewhere in this memorial number, Professor Moulton has described Dr. Chamberlin's working method, the method of multiple working hypotheses, and has placed it in contrast with that more facile but misleading method of the ruling hypothesis, with which most investigators more or less consciously struggle, but to which too many succumb. Chamberlin did not succumb to it. He consciously and conscientiously fought it in his own processes of research, and attacked its products in the work of others when fallacies were demonstrated by critical tests. He placed truth above all personal preference or authority. He also recognized that the truths we see are but partial truths and that they must be amended or even discarded and replaced, as new facts, new understandings appear.

In his fields of thought there grew many beautiful flowers. Some, though beautiful, were unfruitful; they could not live on. Others, having in larger or smaller measure inherited the seed of truth, must live and evolve. Frequently the distinction was difficult to recognize. It often happened that from one and the same root there sprang two or more shoots, mostly barren, but one productive. He watched, cultivated, pruned them all, and gathered the fruitful seeds. His garden was never his alone; it is ours also. He has left our garden more beautiful, richer in the flowers of reality. Let us cultivate it as he did.

THOMAS CHROWDER CHAMBERLIN AS A PHILOSOPHER

F. R. MOULTON
Chicago

IF THE word *philosopher* means *a lover of wisdom*, as its origin would imply, then Thomas Chrowder Chamberlin was a great philosopher. From boyhood he had a passion for inquiring deeply into the meaning of things, particularly in the physical and the biological worlds, and in his mature years he turned from the high honors attached to the presidency of a great university in order to be able to devote more of his time and energies to research.

Although the term *philosopher* has come to have a more restricted meaning than *a lover of wisdom*, I wish to use it here in its broader etymological sense. In the narrower sense it would not justly apply to Dr. Chamberlin; at least, he did not devote himself technically to dialectics and metaphysics. He was deeply interested, however, in arriving at sound conclusions respecting the nature of the physical universe about us, and if he were to be classified as a philosopher, he would be called a pragmatist.

Scientists are often extraordinarily naïve respecting the processes they employ and the nature of the conclusions they reach. It has been a common error for them to confuse their inferences from sense impressions with the sense impressions themselves and, consequently to believe that their conclusions are of the most certain character. For example, a thousand generations of men did not doubt that they saw the sun set, but now we know that this conclusion was simply an erroneous inference from their sense impressions. The rocks are apparently solid, but actually very porous, at least in the atomic and electronic sense; the earth seems to be stationary, but is in reality moving forty times faster than a rifle bullet; wood appears to be destroyed by burning, but actually it is only transformed chemically; and so on through the whole range of our sense impressions, usually "things are not what they seem." Countless examples can be cited in which the original immediate conclusions from ex-

perience have been found to be not in harmony with wider experience. These abandoned interpretations teach us, or at least should teach us, that all our interpretations of phenomena are inferences; and we should realize, consequently, that the validity of our methods of using experiential data is fully as important as the reliability of the data on which our conclusions are based.

Early in his life, Dr. Chamberlin began to reflect upon scientific methodology. Although, as the son of a minister, he was reared under the influence of the deductive theology of eighty years ago, and although his college course was the classical, yet, while still a young man, he reasoned himself out of syllogistic medieval formalism to the inductive methods of, for example, Darwin. Instead of attempting to trace out the intellectual evolution he underwent between his college days and his mature years, it will be more profitable to limit our attention to those methods of scientific investigation which he advocated in formal statements during his later years, and which he put into practice with extraordinary success.

In 1892, Dr. Chamberlin read a paper before the Society of Western Naturalists on scientific methodology; he enlarged this paper and published it, in 1897, in the *Journal of Geology* under the title, "The Method of Multiple Working Hypotheses." In 1904, he delivered an address at the International Congress of Arts and Sciences, in St. Louis, on "The Methods of the Earth-Sciences." This address was published in the *Popular Science Monthly*, November, 1904. Although these formal presentations of his ideas are clear and forceful, yet his concrete applications of them in his books and scientific memoirs are even more instructive. In addition to these sources of information respecting his point of view and methods of work, I had for thirty years the priceless privilege of being intimately associated with him in adventures in unexplored regions of space and of time. Not often is there a combination of circumstances so favorable for interpreting the scientific methods of a great man.

The methods of science, like styles in dress, appear to come and go in cycles. At one period it is the fashion to emphasize the accumulation of observational data and to disparage attempts at their interpretation. In extreme cases, scientists take pride in not having theories. Such was the condition that prevailed thirty years ago in

astronomy, and probably in other physical sciences as well. Dr. Chamberlin called this the "method of colorless observation." In the vigorous terms he was wont to use, he insisted that, unless the quest for truth is inspired by preliminary glimpses of a synthesis of observational data under some rational theory and by hopes of ultimately attaining it, the curiosity atrophies, the zeal for accomplishment declines, the critical faculties fail to distinguish the unimportant from the important, and so-called research becomes sterile.

The opposite extreme is the fashion of hastily constructing theories, one after another, for explaining each new phenomenon that is observed. When this is the mode, as it has been for the last few years, scientists launch hypotheses without pausing to determine whether their various parts are consistent with one another, or whether the hypotheses themselves are not flatly contradictory to other accepted theories or to data other than those which they were invented to explain. These methods become the fashion during periods of frenzied research when priority is more highly prized than soundness and permanence.

There is a related fashion in science which Dr. Chamberlin called the "method of the ruling theory." In this method, a theory once formulated becomes the chief center of interest. Instead of making an exhaustive examination of the phenomena in its field and in domains closely related to it, the investigator has a tendency to limit himself to the facts that are likely to support his theory. Data that are clearly in harmony with it take on in his mind an undeserved importance; those that are clearly contradictory to it are neglected as unimportant or probably subject to unknown errors. The investigator naturally directs his search for new phenomena to support his theory, and unwittingly he presses theory and observational data into harmony.

The defects of the "method of the ruling theory" are obvious and grave. It is strange that what is so obvious in general often is not perceived in particular cases. Every scientist can recall instances in the history of his science, and perhaps within the domain of his own observations, in which some theory that may originally have been an engine of progress long inertly blocked the road to further ad-

vances. Such were many of the dicta of Aristotle. Such was the Laplacian theory of the origin of the planets, and such was the Helmholtzian theory of the source of stellar energies. Such have been all the great "ruling theories."

In pointing out the dangers of adopting ruling theories, Dr. Chamberlin did not limit himself to geology; the range of his interests was prodigious. For example, as is well known, he did not look with favor on Einstein's theory of relativity. One of the reasons for his attitude was that he felt that its expositors are frequently mystical or unduly attracted by paradoxes and bizarre conclusions. Naturally these characteristics of some of its supporters irritated him, for such mental habits have seldom led to results of permanent value. But what offended him most was that a considerable number of workers in physical science seem to have made the theory of relativity a "ruling theory." They seemed to him to have not only deliberately set out to confirm it, but also to have exploited in a sensational manner observational results that were only roughly consistent with special deductions from it. Dr. Chamberlin was quite willing to accept the judgment of mathematicians respecting the mathematical beauty of the theory of relativity, and he was ready to admit that several classes of phenomena, so far as they can be quantitatively determined, are simply co-ordinated under it. But he was not willing to admit that a theory supported by so really meager and, in some cases, doubtful observational evidence had reached the ultimate stage at which alternative hypotheses could safely be ignored; or that it could be followed without any hesitation or misgivings into the most far-reaching conclusions, especially when less bizarre conclusions are also in harmony with it. The sanity of his position on this question is supported by the whole history of philosophy, which is strewn with the remains of abandoned systems that similarly once held sway over the intellectual world. And if current press reports may be relied on, Einstein is already abandoning the Riemannian geometry which led some of his followers to the results with which they took obvious delight in startling an uninstructed world. There can be little doubt that in this field, as in all others, there will be no finality or last word.

Apparently these remarks wander from the subject under dis-

cussion, but actually they illustrate a certain characteristic of Dr. Chamberlin's mind. He was never willing to adopt any point of view simply because it was the fashion, nor to follow a "ruling theory" and not give all other possibilities full consideration. No one else knows so well as I do the thoroughness with which he examined the cosmogony of Kant, the Laplacian theory and its modifications, and the meteoritic hypothesis, not for the purpose of disproving them but for the purpose of finding how far they would go in explaining all the phenomena to which they are related. No one else knows so well as I do that his every instinct and the habits of a lifetime kept him from ever making the planetesimal hypothesis a "ruling theory." To his last days he never ceased to regard it as a hypothesis to be put to the test in every possible way, rather than as an expression of final truth.

Dr. Chamberlin's scientific philosophy was not negative in character, as might be inferred from the foregoing paragraphs. They have been written largely to throw into emphatic relief the positive aspects of his methods. In his writings he stressed and, what is more important, in his work he employed consistently what he called the "method of multiple working hypotheses." The title of this method almost defines it. As its name implies, it consists in formulating and simultaneously examining every hypothesis that gives any promise of explaining the phenomena in some particular field. On the one hand, it raises the quest for truth above the monotonous level of "colorless observation;" on the other, it avoids subserviency to some "ruling theory."

As Dr. Chamberlin says somewhat metaphorically:

> The investigator thus becomes the parent of a family of hypotheses; and by his parental relations to all is morally forbidden to fasten his affections upon any one. In the very nature of the case, the chief danger that springs from affection is counteracted. Where some of the hypotheses have been already proposed and used, while others are the investigator's own creation, a natural difficulty arises, but the right use of the method requires the impartial adoption of all alike into the working family. The investigator thus at the outset puts himself in cordial sympathy and in parental relations (of adoption, if not of authorship,) with every hypothesis that is at all applicable to the case under investigation. Having thus neutralized so far as may be the partialities of his emotional nature, he proceeds with a certain natural and enforced erectness of

mental attitude to the inquiry, knowing well that some of his intellectual children (by birth or adoption) must needs perish before maturity, but yet with the hope that several of them may survive the ordeal of crucial research, since it often proves in the end that several agencies were conjoined in the production of the phenomena.

How seldom have these precepts been followed in science or philosophy! How different in many cases the results would have been had they been followed! What need there is for them also in social and political sciences, in theology, and in all the varied interests of the human mind!

It would be misleading to imply that the "method of multiple working hypotheses" had not been conceived of and used more or less by many predecessors of Dr. Chamberlin. But it is likely that no one before him formulated the method so clearly, and certainly no one used it more consistently; in fact, only exceptionally has it been used at all. Much more frequently the method of scientists has been to invent a hypothesis to account for certain phenomena, and then avowedly to avoid considering any other explanation of them until absolutely forced to do so. To consider a second hypothesis as a possibility implies that the first is not certain, and most minds, being distressed by uncertainties, remain fully contented under any theory which is not too radically contrary to experience. Dr. Chamberlin, however, never played the ostrich. He held up his head to confront every pertinent fact; indeed, he was always searching for new facts. And having obtained new observational data, he relentlessly brought them to bear on each of his multiple hypotheses in turn. It was these methods that placed him so often in advance of his contemporaries.

When I first became acquainted with Dr. Chamberlin, in 1898, he was in the midst of his studies respecting the cause or causes of the glacial epochs. Early in his scientific career, he had been engaged in geological work in southern Wisconsin, where the stratified and fossiliferous sediments of Paleozoic seas lay buried beneath a thick mantle of glacial drift, brought down from the north by successive sheets of slowly moving ice. The evidence that glaciers a thousand feet in thickness had more than once pushed down over the region was conclusive; the places at which their tongues and lobes had

paused before retreating were marked by great moraines, consisting of fragmented and worn materials transported from far distant sources.

Naturally there were many suggested explanations of the glacial epochs. These theories were of such varied nature as the hypothesis that the axis of the earth has shifted, bringing the glacial regions down from high latitudes; that the periodically varying eccentricity of the earth's orbit is an adequate cause (Croll's theory); that the distance from the earth to the sun has varied; that at various times the earth has passed through particularly cold parts of space; that the rate of the sun's radiation has changed, and that the carbon-dioxide content of the earth's atmosphere has undergone roughly periodic oscillations. Dr. Chamberlin examined all these theories and others by the "method of multiple working hypotheses." By 1898, when I became associated with him, he had been compelled to admit the inadequacy of most of these suggested explanations of the ice ages, and he was then engaged in a study of the early history of the earth's atmosphere in order to determine to what extent its constitution could have varied. The pursuit of that inquiry led us out beyond the earth's atmosphere to other planets and the sun, and even to the myriads of other suns that make up the great aggregate of stars we call our galaxy. It led us back beyond the origin of the earth to the time when our sun and another star, in traversing their paths in our galaxy of stars, passed near each other and gave birth to the planets. Though the quest led out a hundred thousand light-years to the border of our galaxy, and back ten thousand million years to the birth of our planet, yet the causes of the ice ages still remain uncertain. But as a consequence of these searches for an explanation of the glacial epochs, Dr. Chamberlin fixed his name imperishably in science by enormously expanding our ideas of the scope of the cosmic processes in time and in space, and by laying a new foundation for dynamic geology in his theory of the origin of the earth.

The statement that Dr. Chamberlin's writings are difficult to understand has interested me much, because to me his style has always seemed exceptionally clear. I have recently read over many of his papers and have attempted to discover why his reasoning has

sometimes seemed involved and his conclusions obscure. One of the characteristics of clarity of style is certainly thoroughness of organization of material presented. Dr. Chamberlin's arguments and expositions are always models of systematic arrangement, as probably all who have read his writings would agree. His paragraphs have the properties of unity and coherence, and his sentences are varied and well constructed. It has sometimes been stated that he used large or learned words, but as a matter of fact he used words with exceptional discrimination. He frequently employed figures of speech, but they were usually not sudden turns that flash a partial truth or tickle the fancy for the moment; nor did he indulge in hyperboles or paradoxes. His figures of speech are more like the parables of Jesus, though they are not parables in form, in that the more they are considered the more they teach. It was noted in an earlier paragraph that he spoke of working hypotheses as our children, among which we should divide our affections. In his address in St. Louis, in 1904, he introduced his discussion with the following remarkable figure of speech:

> That which passes under the name of earth-science is not all *science* in the strict sense of the term. Not a little consists of generalizations from incomplete data, of inferences hung on chains of uncertain logic, of interpretations not beyond question, of hypotheses not fully verified and of speculation none too substantial. A part of the mass is true science, a part is philosophy, as I would use the term, a part is speculation, and a part is yet unorganized material. However, I like to think of the aggregate, not as an amorphous mixture of science, philosophy, and speculation, but as a rather definite aggregation of these, not unlike the earth itself. The great mass of our subject-material may be regarded as a lithosphere of solid facts. Around this gathers an atmosphere of philosophy, rather dense near the contact zone, but thinning away into tenous speculation in the outer regions. For myself, I like to think of the nucleus as solid and firm throughout, not as a thin fractured crust floating on a fiery liquid of plutonian suggestiveness. I like to think of the philosophic and speculative atmosphere as no mere gas-zone of forty-five miles' depth, as of old, but as an envelope of intense kinetic life, in the denser zone, where the logical molecules touch one another with marvelous frequency, and where there is frictional contact with the solid but rather inert lithosphere. In the outer tenuous zone, the molecular flights are freer and the excursions are without assignable limits. I believe an appropriate atmosphere of philosophy is as wholesome to the intellectual life of our sciences as is the earth's physical atmosphere to the life of the planet. None the less, it must ever be our endeavor to reduce

speculation to philosophy, and philosophy to science. For the perpetuation of the necessary philosophic atmosphere, we may safely trust to the evolution of new problems concurrently with the solution of the old.

To one familiar with the structure and the properties of the earth's atmosphere, the rather extended simile of the foregoing paragraph is enormously rich in content and suggestion.

Why, then, one may ask, have Dr. Chamberlin's writings sometimes been found difficult to understand? He has fully answered the question himself, I think. In referring to the "method of multiple working hypotheses" (*op. cit.*) he wrote:

> An infelicity also seems to attend the use of the method with young students. It is far easier, and apparently in general more interesting, for those of limited training and maturity to accept a simple interpretation or a single theory and to give it wide application, than to recognize several concurrent factors and to evaluate these as the true elucidation often requires. Recalling again for illustration the problem of the Great Lakes basins, it is more to the immature taste to be taught that these were scooped out by the mighty power of the great glaciers than to be urged to conceive of three or more great agencies working successively in part and simultaneously in part and to endeavor to estimate the fraction of the total results which was accomplished by each of these agencies. The complex and the quantitative do not fascinate the young student as they do the veteran investigator.

We are all "young students" and "of limited training and maturity," especially in subjects which we have not studied extensively, and naturally we have difficulty in following the comprehensive mental processes of a master. In this connection, it is worthy of remark that those who have had the greatest success in winning a following among mankind have expressed their theories most positively and with fewest qualifications. In Moses' account of the creation there is no note of uncertainty, nor is there in the words of any of the great religious teachers. As has already been remarked, we desire certainty and finality, and we become confused if we do not get it. Consequently, the scientifically immature often find the application of the "method of multiple working hypotheses" difficult and sometimes obscure, though as a general method it is easy to understand and its merits are obvious.

In his St. Louis address, Dr. Chamberlin discussed also what he called the "method of regenerative hypotheses." This method consists in following out a succession of hypotheses, each a consequence

of the preceding, if not in a strict logical sense at least in its genesis. A chain of hypotheses of this kind often reveals elements of strength or of weakness in the original assumptions that would not be easily perceived in considering them directly. The final conclusion often suggests a revision of the original hypothesis and the construction of a new chain, as Dr. Chamberlin aptly illustrated with Sir George Darwin's theory of tidal evolution. He also made a very interesting illustration in discussing the question of determinism, or, as it is called, the freedom of the will.

Dr. Chamberlin's illustration of the "method of regenerative hypotheses" in connection with the question of the freedom of the will deserves a few comments; and it suggests that I add, also, a few sentences respecting a subject on which, naturally, he did not express himself explicitly in his scientific writings. In discussing determinism he assumed first that the human mind is surrounded by a mixture of reality and of illusion, and that it is capable of detecting error and demonstrating truth; and that, moreover, it has choice and some measure of volitional command over itself. He proceeded to consider physical phenomena, in which we find invariable sequences of events that we commonly call cause and effect. Then he went over into the chemico-biological field, and finally to the mental, in each of which we encounter a similar unvarying sequence of antecedents and consequents. Then the conclusion follows that mental phenomena exhibit the same relation of cause and effect as is found in the inanimate world. But this contradicts the original hypothesis that the human mind has the power of choice, volitional control, and the alternative of reaching truth or falling into error according to its self-directed discrimination.

Instead of arriving by this course of reasoning, as many have done, at the fixed conclusion that the human mind does not have the power to choose, and then taking a sort of melancholy pride in its impotency, he inquired, rather, what final goal would be reached by starting with the opposite hypothesis and pursuing its consequences sequentially by the same method. He found equal difficulties with this alternative initial assumption. Naturally he concluded that we have no basis for the dogmatic assertion that either position is inescapable.

The problem of the freedom of the will lies on the borders of

the field which includes such transcendental questions as those of the real nature of the human spirit, of good and evil, and of an Infinite Power in the universe; and though the human mind may know and feel that this region will be forever beyond its power of effective penetration, yet again and again it struggles to break through its barriers. Naturally such subjects are not topics for casual conversation; they belong to the solitudes of the seashore or the mountains or the stars. Yet on many occasions when our minds had become weary with celestial mechanics, Dr. Chamberlin spoke freely of these things. Possibly the absence of the slightest restraint or diffidence was due to our habit of absolute frankness with each other; perhaps our wide excursions in the interstellar spaces for the time had in effect disembodied our minds. At any rate, in these rare hours Dr. Chamberlin revealed his inmost thoughts as much as one human being can bare himself to another. What he said cannot be reproduced on the printed page, to be read in any place and in any mood in which the reader may chance to be when it reaches his eye. It will be sufficient to say that his optimistic pragmatism compares, on the one hand, with the usual theological dogmatism as a breath from the ocean compares with the stagnant mustiness of a cloister, and that it compares, on the other hand, with barren and cynical skepticism as blossoming prairies compare with the desert.

Rather than to attempt an explicit interpretation of the mind of another in a domain of thought and emotion in which the prejudices of all are strong and narrow, it is preferable to let him speak for himself in a field that is related to it so closely that the reader will feel the comprehensiveness of his grasp of the most fundamental problems with which the human mind struggles. Rejecting absolutism, on the one hand, and hopeless pessimism, on the other, Dr. Chamberlin, in concluding his St. Louis address, expressed his ideas respecting that which we may accept as truth, and the methods we should employ for attaining it, in a paragraph which for breadth of vision and dignity of language has rarely been approached:

> Wherein lie the basal criteria of our sciences? I believe they lie essentially in their working quality. Whatever conforms thoroughly to the working requirements of nature probably corresponds essentially to the absolute truth, though it may be much short of the full truth. That may be accepted, for the

time being, as true which duly approves itself under all tests, as though it were true. Whenever it seems to fail under test in any degree, confidence is to be withdrawn in equal degree, and a rectification of conceptions sought. This may well hold all conceptions, however fundamental, whether they relate to the physical, the vital, or the mental phenomena which the earth presents. Let us entirely abandon the historic effort of the metaphysicians to build an inverted pyramid on an apex of axioms assumed to be incontestable truth, and let us rear our superstructure on the results of working trials applied as widely and as severely as possible. Let us seek our foundation in the broadest possible contact with phenomena. I hold that the working test when brought to bear in its fullest, most intimate and severest forms is the supreme criterion of that which should stand to us for truth. Our interpretative effort should, therefore, be to organize a complete set of working hypotheses for all phenomena, physical, vital and mental, so far as appropriate to our sphere of research. These should be at once the basis of our philosophy and of our science. These hypotheses should be constantly revised, extended and elaborated by all available means, and should be tested continually by every new relation which comes into view, until the crucial trials shall become as the sands of the sea for multitude and their severity shall have no bounds but the limits of human capacity. That which under this prolonged ordeal shall give the highest grounds of assurance may stand to us for science, that which shall rest more upon inference than upon the firmer modes of determination may stand to us for our philosophy, while that which lies beyond these, as something doubtless always will, may stand to us for the working material of the future.

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